PROCEEDINGS

OF

THE ROYAL SOCIETY

OF

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PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XVII. 1889–90. No. 130.

The 107th Session.

GENERAL STATUTORY MEETING.

Monday, 25th November 1889.

The following Council were elected:—

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Sir WILLIAM THOMSON, F.R.S.

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Professor Sir Douglas Maclagan.
The Hon. Lord Maclaren, LL.D.
Rev. Professor Flint, D.D.
Professor Chrystal, LL.D., F.R.A.S.
Thomas Muir, Esq., LL.D.
Sir Arthur Mitchell, K.C.B.

General Secretary—Professor Tait.

Secretaries to Ordinary Meetings.

Professor Sir W. Turner, F.R.S.
Professor Crum Brown, F.R.S.

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Professor James Geikie, LL.D., F.R.S.
W. H. Perkin, Esq., Jun., D.Sc.
A. Beatson Bell, Esq.
The Rt. Hon. Lord Kingsburgh, C.B., LL.D., F.R.S.
John Murray, Esq., LL.D.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council:—

His Grace the DUKE of ARGYLL, K.G., K.T.
The Right Hon. LORD MONCREIFF of Tulliebole, LL.D.

Glissettes of an Ellipse and of a Hyperbola. By Professor Tait. (With a Plate.)

(Read December 16, 1889.)

Last summer, while engaged with some quaternion investigations connected with Dr Plarr's problem (the locus-boundary of the points of contact of an ellipsoid with three rectangular planes) I was led to construct the glissettes of an ellipse. I then showed to the Society a series of these curious curves, drawn in my laboratory by Mr Shand, who had constructed for the purpose a very true elliptic disc of sheet brass. I did not, at the time, think it necessary to print my paper; but, after the close of the session, I made the curious remark that precisely the same curves can be drawn each as a glissette of its own special hyperbola. This double mode of sliding generation of the same curve seems to possess interest. It is somewhat puzzling at first, since the ellipse turns completely round, while the hyperbola can only oscillate. But a little consideration shows the cause of the coincidence.

Let O be the origin, C any position of the centre of the ellipse, CA that of the major axis, and P the corresponding position of the tracing point. This does not require a figure.

Then it is easy to see that if ϕ be the inclination of OC to one of the guides, θ that of CA to the same, we have

$$\sqrt{a^2\cos^2\theta + b^2\sin^2\theta} = \sqrt{a^2+b^2}\cos\phi .$$

But this gives

$$\sqrt{a^2\cos^2\phi - b^2\sin^2\phi} = \sqrt{a^2-b^2}\cos\theta ,$$

which is the corresponding relation for the hyperbola. In fact the one equation is changed into the other by changing the sign of b^2, and interchanging the angles θ and ϕ.

Let the polar coordinates of the tracing point, referred to the centre of the ellipse and the major axis, be r, α, we obtain a position of P by the broken line OC, CP; their lengths being $\sqrt{a^2+b^2}$, r, and their inclinations to the guide ϕ, $\theta+\alpha$, respectively.

If we now turn the guides through an angle α, and use a hyperbola whose axes are to those of the ellipse respectively as $r:\sqrt{a^2-b^2}$; and consider the curve traced by a point Q in its

plane, whose central polar coordinates are $\sqrt{a^2+b^2}, -\alpha$; the position of the point Q is given by the broken line OC′,C′Q. Of these OC′ is equal and parallel to CP, while C′Q is equal and parallel to OC. Thus the points Q and P coincide.

In fact the motion of either is the resultant of two circular motions, one of which is complete (viz., θ, which has all values from 0 to 2π), the other reciprocating $\left(\text{viz., } \phi, \text{ which varies between } \sin^{-1}(b/\sqrt{a^2+b^2}) \text{ and } \sin^{-1}(a/\sqrt{a^2+b^2})\right)$. But, in the case of the ellipse, the centre has the reciprocating motion; while, in the hyperbola, it describes the complete circular path.

Mr Shand has constructed a hyperbolic disc, comprising a considerable portion of each of the branches of the curve, and it gives very fair glissettes. It is very curious to watch the proper point of the hyperbola gliding over the curve already traced by the ellipse. But this apparatus is not so easily managed as is the elliptic disc, so that the figures in the plate were drawn by means of the latter, and reproduced on a diminished scale by photolithography.

To exhibit, by a few forms, as completely as possible the general nature of these glissettes, I selected a series of tracing points equidistant from the centre of the ellipse, and situated within and on the boundaries of the various regions, to each of which belongs a special form. For this purpose I traced the curve formed by successive positions of the instantaneous centre of rotation on the disc. The disc, with this curve on it, is represented in the upper central figure. The equation of the curve is

$$\frac{b^2x^2-a^2y^2}{b^2x^2+a^2y^2} = \frac{\sqrt{a^2+b^2}\sqrt{x^2+y^2}}{a^2-b^2}.$$

It is easily traced as follows. Draw the ellipse whose semiaxes parallel to x and y respectively are

$$\frac{a^2-b^2}{\sqrt{a^2+b^2}} \quad\text{and}\quad \frac{a}{b}\,\frac{a^2-b^2}{\sqrt{a^2+b^2}};$$

diminish every radius vector in proportion to the cosine of double the angle vector; and then diminish the ordinates in the ratio $b : a$, so that the ellipse itself becomes a circle.

In the disc from which the glissettes were drawn, a (rather more than a foot in length) was made double of b.

This equation suggested, as a useful distance of the tracing point from the centre, the quantity

$$\frac{a^2-b^2}{2\sqrt{a^2+b^2}};$$

and accordingly the points O, A, B, C, D, E, F were taken on the corresponding circle. The glissettes of B and D, of course, have cusps :—and it is interesting to study the changes of form from one to the next of the seven just named. Two groups of figures give the glissettes of successive points on each of the axes separately, viz., G, O, K, M on the major axis, and J, F, L, N on the minor. Of these K and L have cusps. The figures G, H, J were drawn to show how the glissettes of points near the centre approximate to the (theoretical) four cusps which belong to the path of the centre itself, the finite circular arc described four times over during a complete rotation of the ellipse. The point P was chosen as close as possible to the intersection of the ellipse and the centrode.

The locus of the instantaneous axis in the guide-plane is of no special interest. It is easy to construct it geometrically from its polar equation, which may be written generally as

$$r(2\sqrt{a^2+b^2}-r)=4a^2b^2/(a^2+b^2)\sin^2 2\theta,$$

or in the present special case

$$r(\sqrt{5}a-r)=4a^2/5\sin^2 2\theta.$$

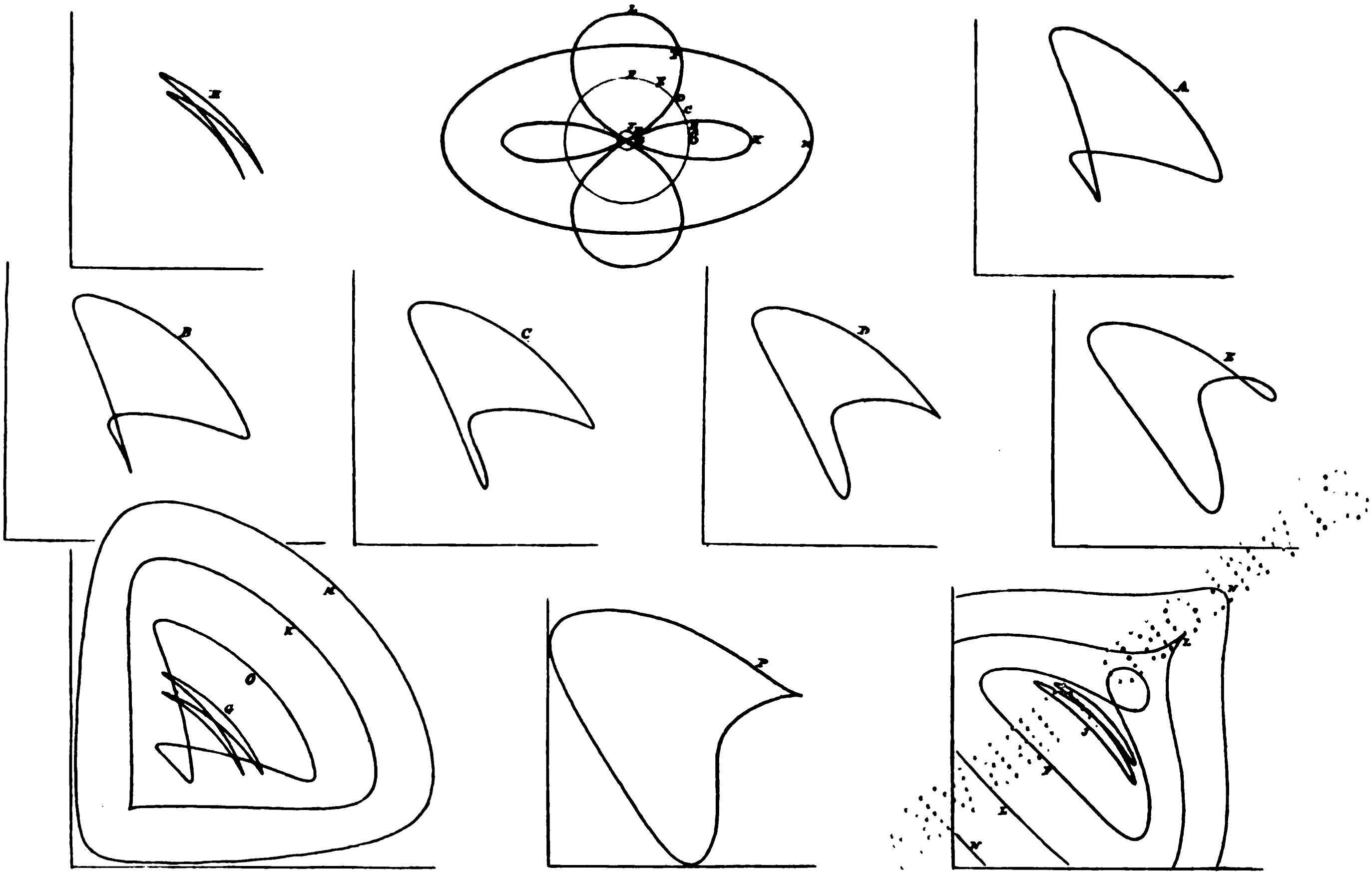
It is an ovoid figure, symmetrically situated between the guides, with its blunter end turned from the origin.

The equation of the glissettes is found by eliminating θ between the equations

$$x=\sqrt{a^2\cos^2\theta+b^2\sin^2\theta}\;+\;r\cos(\theta+a),$$
$$y=\sqrt{a^2\sin^2\theta+b^2\cos^2\theta}\;+\;r\sin(\theta+a).$$

This seems to lead to a relation of the 12th degree in x and y; but it must contain a spurious factor, as Professor Cayley informs me the final result ought to be of the 8th degree. And in fact we see at once that, if the tracing point be at a *very* great distance from the centre (in comparison with the major axis of the ellipse) the glissette will consist practically of four circles, with centres in the four quadrants between the guide-lines.

GLISSETTES OF AN ELLIPSE OR HYPERBOLA



Observations upon the Structure of a Genus of Oligochæta belonging to the Limicoline Section. By Frank E. Beddard, M.A., F.R.S.E., *Prosector and Davis Lecturer to the Zoological Society of London.*

(Read December 16, 1889.)

(*Abstract.*)

The present paper refers to a number of specimens of *Moniligaster*, some of which belong to the species *Moniligaster Barwelli* already briefly described by myself;* others (*one* other at least) are possibly referable to a distinct species. I do not, however, give this species a name, principally for the reason that it will very possibly turn out to be identical with one or other of the species recently noted by Professor A. G. Bourne from the Nilgiris and Shevaroys. I am not at all certain that *M. Barwelli* is not identical with his *M. minutus.* The present paper contains a fuller account of the reproductive organs than I have yet given, but unfortunately it will be still found to be incomplete in certain particulars; some of these gaps, *e.g.*, the clitellum and the egg-sacs, are filled up by Bourne's paper, assuming of course that the characters of these organs as given by Bourne will turn out to be *generic*, which is likely. The present paper contains some account of the other organs of the body, which has not yet been published.

As I have taken pains in common with other naturalists to point out that the Oligochæta cannot be divided into "*Limicolæ*" and "*Terricolæ*," it may seem illogical to speak of a "Limicoline Section." As, however, I regard most earthworms as forming a distinct group marked off from other Oligochæta, it is convenient to have an expression for Oligochæta which agree in *not* being "*Terricolæ;*" for this purpose the terms "Limicolæ" or "Limicoline Section" are made use of simply as a matter of convenience.

I consider (in opposition to Dr Rosa) that *Moniligaster* is not an earthworm except in habit; its affinities with earthworms are chiefly shown in the presence of gizzards, in the thickness of body

* I give no references to literature in this Abstract; they will be found in the complete paper.

walls and septa, particularly in the anterior region of the body, and in the corresponding vascularity of the integumental layers.

It differs from *all* earthworms in the following points:—

1. The vas deferens is single on each side; it only occupies a single segment, or at most two.

2. There is only a single pair of testes, which may be in segment IX.

3. The sperm-sacs are a single pair with a simple cavity, *i.e.*, not divided up by trabeculæ.

4. The atrium opens on to X/XI; its structure is that of the atrium of *Rhynchelmis.*

5. The oviduct opens into segment XI.

6. The clitellum occupies segments X–XIII.

7. The egg-sacs are very large, occupying about three segments. In these points it approaches various Limicoline genera.

The following are the principal facts in the anatomy of my species of *Moniligaster*. Those statements marked with a dagger (†) are made for the first time in the present paper:—

†1. The *prostomium* is very small, not extending on to the peristomial segment.

2. The *setæ* are strictly paired, and entirely upon the ventral surface of the body.

†3. The *setæ* are not peculiar in shape, but like those of earthworms; only the anterior pairs are larger than the posterior.

†4. There are no *penial* or *copulatory setæ* (?).

†5. *Dorsal pores* are present.

†6. The *mesenteries* separating segments V/VI, VI/VII, VII/VIII, VIII/IX are very much thickened.

†7. The *hearts* are in segments VI–XIV, and are of large size.

†8. The *alimentary tract* begins with a buccal cavity leading into a pharynx; the *pharynx* is well developed; the *œsophagus* is lined with a cuticle, it is very wide; the *gizzards* are three* in number, and lie in segments XIV–XVI; there are no (?) *calciferous glands.*

†9. The *nephridia* commence in segment V; each has a saccular diverticulum.

* In one specimen, *not M. Barwelli*, the gizzards are situated further back, but I am not sure how many there are, and where they begin.

†10. The *testes* are either (*M. Barwelli*) in segment IX, attached to posterior wall, or else in segment X, attached to front wall.

11. The *sperm sacs* (one pair) are in segments IX or X—in correspondence with position of testes; their cavity is undivided.

†12. The *vas deferens funnels*, in accordance with the varying position of the testes, open into IXth or Xth segment.

13. The *atrium* opens between segments X/XI; it has precisely the structure of the atrium of *Rhynchelmis*.

†14. The *oviducts* are in segment XI; in the individual which probably belongs to a species distinct from *M. Barwelli*, the oviducal funnel is spread along the anterior face of the septum separating segments XI/XII.

15. The spermathecæ are a single pair in VIII; each is a small sac with a very long coiled duct.

These points are in my opinion sufficient to render it necessary to regard *Moniligaster* as the type of a distinct family, not, as Rosa believes, of the Terricolæ, but *as equal to* the Terricolæ, Lumbriculidæ, &c. This family has evident relations on the one hand to the Terricolæ, and on the other to the Lumbriculidæ; its affinities however, to any one family of the Limicolæ are not marked. These matters are discussed in some detail in the paper, and form its concluding portion.

On Self-conjugate Permutations. By Thomas Muir, M.A., LL.D.

(Received Sept. 3. Read December 16, 1889.)

1. The conception of conjugate permutations appears to be due to Rothe.* The definition may be expressed thus:—*Two permutations of the numbers* 1, 2, 3, . . . , n *are called conjugate when each number and the number of the place which it occupies in the one permutation are interchanged in the case of the other.* For example, the permutations

$$3,\ 8,\ 5,\ 10,\ 9,\ 4,\ 6,\ 1,\ 7,\ 2 \qquad (A)$$
$$8,\ 10,\ 1,\ 6,\ 3,\ 7,\ 9,\ 2,\ 5,\ 4 \qquad (B)$$

* "Ueber Permutationen, in Beziehung auf die Stellen ihrer Elemente," *Sammlung combinatorisch-analytischer Abhandlungen*, herausg. v. C. F. Hindenburg, ii. pp. 263–305.

are conjugate, because 3 is in the 1st place of (A) and 1 is in the 3rd place of B, and so on in every case. A permutation may be conjugate with itself. Thus the permutation

$$6\ 3\ 2\ 4\ 5\ 1$$

which has 6 in the 1st place, has also 1 in the 6th place, and so on. A self-conjugate permutation may consequently be defined independently, as one in which each element is either in its original position or has taken part in one interchange.

Conjugate permutations are identical with those which Jacobi* calls *reciprocal*, his definition being that two permutations are so called when the performance of the one upon the other gives rise to the primitive permutation. For example, 3 5 2 1 4 and 4 3 1 5 2 are reciprocal permutations, because

$$(35214)\ (43152) = 12345.$$

In regard to self-conjugate permutations, there is an interesting unsettled question which Rothe raised, viz., as to the number of them corresponding to any particular number of elements. As a partial solution he gave the difference-equation which it satisfies, but nothing further. No proof even of the equation was given, and it is the only result so left in his paper. Our present purpose is to furnish a full solution of the problem.

2. *The number of self-conjugate permutations of the elements* 1, 2, 3, . . . , n *is*

$$1 + C_{n,2} + \tfrac{1}{2} \cdot C_{n,2}C_{n-2,2} + \tfrac{1}{2} \cdot \tfrac{1}{3} \cdot C_{n,2}C_{n-2,2}C_{n-4,2} + \ \dots$$

where as usual $C_{n,r}$ *stands for* $n(n-1)\dots(n-r+1)/1\,.\,2\,.\,3\,\dots\,r$.

This is best established as it was first obtained, viz., by classifying the instances of self-conjugateness, and then making a census of the classes. A basis of classification exists in the varying number of elements retaining their primitive positions in the permutations. Taking this basis, we see at once that we have to consider in order the classes

* "De formatione et proprietatibus determinantium," *Crelle's Journ.*, xxii. p. 287.

1. Where *no* element is changed in position.
2. Where *two* are changed.
3. Where *four* . . . .
4. Where *six* . . . . .

. . . .

In the first class there is manifestly only 1, viz., the primitive permutation. In the second class there are as many as there are different pairs of elements, viz., $C_{n,2}$ for example,

$$2, 1, 3, 4, 5, \ldots, n, \qquad 3, 2, 1, 4, 5, \ldots, n.$$

In the case of the third class we have to find how many pairs of pairs are possible, each having the four elements involved all different. For the first of the two pairs we have, as has just been seen, $C_{n,2}$ to choose from; for the second there are only $C_{n-2,2}$ to choose from, because there are two fewer elements to make the pairs of; consequently the number required is $\frac{1}{2}C_{n,2}C_{n-2,2}$, the $\frac{1}{2}$ being due to the fact that the order in which the two pairs are taken is immaterial. In the case of the fourth class, we have to ascertain how many triads of pairs are possible, each having the six elements involved all different; and the result is similarly found to be $\frac{1}{3!}C_{n,2}\,C_{n-2,2}\,C_{n-4,2}$. The remaining classes manifestly follow the same law, consequently the proposition is established.

3. *If* U_n *stand for the number of self-conjugate permutations of the elements* 1, 2, 3, . . . , n, *then*

$$U_n = U_{n-1} + (n-1)U_{n-2}.$$

If, having examined the case of the elements $1, 2, 3, \ldots, (n-1)$ we bring our nth element n to join them, it is clear that we have two possibilities to consider, viz. (1) the element n remaining in its place, (2) the said element suffering interchange with one of the others. Now, when it remains in its own proper position, the number of self-conjugate permutations is unaltered from the previous case, that is to say, is U_{n-1}; and when it is taken with one of the $n-1$ other elements to form a pair for interchange, there will arise, by reason of the remaining $n-2$ elements, U_{n-2} self-conjugate permutations, that is to say, in all $(n-1)\,U_{n-2}$. We have, consequently, as was to be proved,

$$U_n = U_{n-1} + (n-1)U_{n-2}.$$

As there is evidently 1 self-conjugate permutation when $n=1$, and 2 when $n=2$, the first ten values U_n are

$$\begin{array}{ll} U_1 = 1, & U_6 = 76, \\ U_2 = 2, & U_7 = 232, \\ U_3 = 4, & U_8 = 764, \\ U_4 = 10, & U_9 = 2620, \\ U_5 = 26, & U_{10} = 9496. \end{array}$$

4. The expression first got for the number of self-conjugate permutations ought, of course, to satisfy this difference-equation. To show directly that it does, it is best in the first place to simplify it a little. The typical term

$$= \frac{1}{r!} C_{n,2}\, C_{n-2,2}\, C_{n-4,2} \ldots \ldots C_{n-2r+2,2}\,,$$

$$= \frac{1}{r!} \frac{n(n-1)}{1\,.\,2} \cdot \frac{(n-2)(n-3)}{1\,.\,2} \cdot \frac{(n-4)(n-5)}{1\,.\,2} \cdots \frac{(n-2r+2)(n-2r+1)}{1\,.\,2}$$

$$= \frac{n(n-1)(n-2)\ \ldots\ (n-2r+1)}{r!\,2^r},$$

$$= \frac{n(n-1)(n-2)\ \ldots\ (n-2r+1)}{(2r)!} \times \frac{(2r)!}{r!\,2^r}$$

$$= C_{n,2r}\,(1\,.\,3\,.\,5\ \ldots\ 2r-1)\,.$$

The whole expression consequently becomes

$$1 + 1\,.\,C_{n,2} + 1\,.\,3C_{n,4} + 1\,.\,3\,.\,5C_{n,6} + 1\,.\,3\,.\,5\,.\,7C_{n,8} + \ \ldots$$

Taking this for U_n we have

$$\begin{aligned} U_{n-1} + (n-1)U_{n-2} &= 1 + 1\,.\,C_{n-1,2} + 1\,.\,3C_{n-1,4} + 1\,.\,3\,.\,5C_{n-1,6} + \ \ldots \\ &\quad + (n-1)\{1 + 1\,.\,C_{n-2,2} + 1\,.\,3C_{n-2,4} + 1\,.\,3\,.\,5C_{n-2,6} + \ldots\} \\ &= 1 + 1\,.\left\{ C_{n-1,2} + (n-1) \right\} + 1.3 \left\{ C_{n-1,4} + \frac{n-1}{3} C_{n-2,2} \right\} \\ &\quad + 1.3.5 \left\{ C_{n-1,6} + \frac{n-1}{5} C_{n-2,4} \right\} + \ldots \end{aligned}$$

But the general term on the right here

$$= 1\,.\,3\,.\,5\ \ldots\ .\,(2r-1) \left\{ C_{n-1,2r} + \frac{n-1}{2r-1} C_{n-2,2r-2} \right\},$$

$$= 1\,.\,3\,.\,5\ \ldots\ .\,(2r-1) \left\{ C_{n-1,2r} + C_{n-1,2r-1} \right\},$$

$$= 1\,.\,3\,.\,5\ \ldots\ .\,(2r-1)\ C_{n,2r}\,.$$

It therefore follows that

$$U_{n-1}+(n-1)U_{n-2}=1+1\,.\,C_{n,2}+1\,.\,3C_{n,4}+1\,.\,3\,.\,5C_{n,6}+\,.\,.\,.\,.$$
$$=U_n\,.$$

5. To every problem regarding the permutations of 1,2,3, . . . , n, there corresponds a problem regarding the terms of a determinant. What problem on determinants, then, corresponds to the problem here dealt with? The answer is most easily perceived if the elements of a self-conjugate permutation be considered the column-numbers of the determinant, and as such be suffixed to the row-numbers 1, 2, 3, . . . , n, taken always in this the natural order. For example, if we combine the row-numbers 1 2 3 4 5 6 with the self-conjugate permutation 3 6 1 4 5 2 of the column-numbers, we obtain

$$1_3 2_6 3_1 4_4 5_5 6_2 ,$$

a term of the determinant

$$|\, 1_1 2_2 3_3 4_4 5_5 6_6 \,| ,$$

and at once see that it possesses the property of remaining unaltered when the row and column numbers of every factor of it are interchanged—that, in fact, such an interchange merely alters the *order* of its six factors, 1_3, 2_6, 3_1, ... That this is the characteristic property of such a combination of row-numbers in the natural order with column-numbers in any order constituting a self-conjugate permutation is evident on recurring to the definition with which we started. The same property may be expressed with regard to the vertical line notation for determinants, by saying that the elements of the determinant which compose the term are cut symmetrically by the main diagonal. Our problem is thus transformed into finding the number of terms of a determinant of the nth order, which are such that the n elements taken to form any one of them are in the square array distributed symmetrically about the main diagonal. Terms of this kind may fitly be called self-conjugate; and, generally, the word conjugate may be used in regard to the terms of a determinant exactly as in regard to permutations. We may even go further, and apply it to the elements, calling $a_{r,s}$, $a_{s,r}$ conjugate; in which case, two terms could be defined as conjugate when the elements of the one were conjugate

with the elements of the other, and a self-conjugate term is one into which there might enter self-conjugate elements and pairs of conjugate elements.

6. Developing any determinant

$$\begin{vmatrix} 11 & 12 & 13 & 14 & 15 \\ 21 & 22 & 23 & 24 & 25 \\ 31 & 32 & 33 & 34 & 35 \\ 41 & 42 & 43 & 44 & 45 \\ 51 & 52 & 53 & 54 & 55 \end{vmatrix}$$

in terms of binary products of the elements of the first row and of the first column, we have the unique product

$$11 \begin{vmatrix} 22 & 23 & 24 & 25 \\ 32 & 33 & 34 & 35 \\ 42 & 43 & 44 & 45 \\ 52 & 53 & 54 & 55 \end{vmatrix},$$

$n-1$ products of the type

$$12.21 \begin{vmatrix} 33 & 34 & 35 \\ 43 & 44 & 45 \\ 53 & 54 & 55 \end{vmatrix},$$

and $(n-1)(n-2)$ products of the type

$$12.31 \begin{vmatrix} 23 & 24 & 25 \\ 43 & 44 & 45 \\ 53 & 54 & 55 \end{vmatrix}.$$

Now each of the products of the first two types consists of two factors, one manifestly self-conjugate, and the other a determinant of a lower order than the original determinant, but otherwise quite similar to it. On the other hand, in the third typical product, the first factor 12.31 is not self-conjugate, and the second factor cannot furnish the conjugate elements 21, 13. These considerations give us at once the equation

$$U_n = U_{n-1} + (n-1)U_{n-2}.$$

7. *In a zero-axial determinant of the* nth *order*, $U_n = 0$ *when* n *is odd, and* $= 1 . 3 . 5 \ldots . (n-1)$ *when* n *is even.*

Since here $11 = 0$, the unique product just referred to vanishes. Consequently the difference-equation in this case becomes

$$U_n = (n-1)U_{n-2} ,$$

Further, in such a determinant $U_1 = 0$. Therefore

$$0 = U_3 = U_5 = . \;.\;.\;.$$

Again $U_2 = 1$, therefore

$$U_4 = 3 . 1, \qquad U_6 = 5 . 3 . 1 , \;.\;.\;.\;.$$

8. Returning now to the general determinant, and expanding it in terms of zero-axial determinants, we obtain

$$\begin{vmatrix} 11 & 12 & 13 & 14 & 15 \\ 21 & 22 & 23 & 24 & 25 \\ 31 & 32 & 33 & 34 & 35 \\ 41 & 42 & 43 & 44 & 45 \\ 51 & 52 & 53 & 54 & 55 \end{vmatrix}$$

$$= a_{11}a_{22}a_{33}a_{44}a_{55} + \Sigma a_{11}a_{22}a_{33}\begin{vmatrix} . & 45 \\ 54 & . \end{vmatrix} + \Sigma a_{11}a_{22}\begin{vmatrix} . & 34 & 35 \\ 43 & . & 45 \\ 53 & 54 & . \end{vmatrix}$$

$$+ \Sigma a_{11}\begin{vmatrix} . & 23 & 24 & 25 \\ 32 & . & 34 & 35 \\ 42 & 43 & . & 45 \\ 52 & 53 & 54 & . \end{vmatrix} + \begin{vmatrix} . & 12 & 13 & 14 & 15 \\ 21 & . & 23 & 24 & 25 \\ 31 & 32 & . & 34 & 35 \\ 41 & 42 & 43 & . & 45 \\ 51 & 52 & 53 & 54 & . \end{vmatrix},$$

where under the first Σ is included $C_{5,3}$ terms, under the second $C_{5,2}$, and so on. And since the number of self-conjugate terms on the one side must be the same as the number on the other, there at once results with the help of the theorem of the preceding paragraph,

$$\begin{aligned} U_n = 1 + C_{n,n-2} . 1 + C_{n,n-3} . 0 + C_{n,n-4} . 3 . 1 + C_{n,n-} \;. 0 \\ + C_{n,n-6} . 5 . 3 . 1 + \;.\;.\;. \\ = 1 + 1 . C_{n,2} + 1 . 3C_{n,4} + 1 . 3 . 5C_{n,6} + \;.\;.\;. \end{aligned}$$

as before found.

On a Rapidly Converging Series for the Extraction of the Square Root. By Thomas Muir, LL.D.

(Received October 20. Read December 16, 1889.)

It is well known that the square root of any commensurable number N is expressible in the form

$$A+\frac{1}{\dot{a}_1}+\frac{1}{a_2}+\cdots\cdots+\frac{1}{a_z}+\frac{1}{2\dot{A}}+\cdots,$$

and that the $(z+1)^{\text{th}}$ convergent to this is

$$\frac{K(A,a_1,a_2,\ldots\ldots,a_z)}{K(a_1,a_2\ldots\ldots,a_z)}\quad\text{or}\quad A+\frac{K(a_2,\ldots,a_z)}{K(a_1,a_2,\ldots,a_z)},$$

where the continuant notation K( ) is explained by the example

$$K(a_1,a_2,a_3)=\begin{vmatrix} a_1 & 1 & . \\ -1 & a_2 & 1 \\ . & -1 & a_3 \end{vmatrix}.$$

Subtracting the $(z+1)^{\text{th}}$ convergent from the $2(z+1)^{\text{th}}$ we have

$$\frac{K(A,a_1,a_2,\ldots,a_z,2A,a_1,a_2,\ldots,a_z)}{K(a_1,a_2,\ldots,a_z,2A,a_1,a_2,\ldots,a_z)}-\frac{K(A,a_1,a_2,\ldots,a_z)}{K(a_1,a_2,\ldots,a_z)}$$

$$=\frac{K(A,a_1,\ldots,a_z,2A,a_1,\ldots,a_z)K(a_1,\ldots,a_z)-K(a_1,\ldots,a_z,2A,a_1,\ldots,a_z)K(A,a_1,\ldots,a_z)}{K(a_1,\ldots,a_z,\,2A,a_1,\ldots,a_z)K(a_1,\ldots,a_z)}$$

$$=\frac{(-1)^zK(a_1,\ldots,a_z)}{K(a_1,\ldots,a_z,2A,a_1,\ldots,a_z)K(a_1,\ldots,a_z)},$$

$$=\frac{(-1)^z}{K(a_1,\ldots,a_z,2A,a_1,\ldots,a_z)}\,.$$

Similarly, on subtracting the $2(z+1)^{\text{th}}$ convergent from the $4(z+1)^{\text{th}}$ there is obtained

$$-\frac{(-1)^{2z+1}}{K(a_1,\ldots,a_z,2A,a_1,\ldots,a_z,2A,a_1,\ldots,a_z,2A,a_1,\ldots,a_z)};$$

and so on generally, the numerators of the successive differences being evidently

$$(-1)^z,\ (-1)^{2z+1},\ (-1)^{4z+3},\ (-1)^{8z+7},\ldots\ldots,$$

and any denominator being got by taking 2A for its middle element, and writing the elements of the previous denominator both in front of this 2A and after it.

Combining these results, we obtain the rather notable identity

$$A+\frac{1}{\underset{*}{a_1}+}\,\frac{1}{a_2+}\cdots+\frac{1}{a_s+}\,\frac{1}{\underset{*}{2A}+}\cdots$$

$$=A+\frac{K(a_2,\ldots,a_s)}{K(a_1,a_2,\ldots,a_s)}+\frac{(-1)^s}{K(a_1,\ldots,a_s\,2A,a_1,\ldots,a_s)}$$

$$-\frac{1}{K(a_1,\ldots,a_s\,2A,\,a_1\ldots,a_s\,2A,\;a_1,\ldots,a_s\,2A,\,a_1,\ldots,a_s)}$$

$$-\frac{1}{K(a_1,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,a_s)}$$

$$-\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.$$

The rapidly converging character of the right-hand side is unmistakable. There are, however, one or two transformations possible upon it which considerably enhance its value.

In the first place, each denominator after $K(a^1,\ldots,a_s)$ can be resolved into factors, viz.,

$$K(a_1,\ldots,a_s\,2A,a_1,\ldots,a_s)=2\,K(a_1,\ldots,a_s)K(a_1,\ldots,a_s\,A)\,,$$

$$K(a_1,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,a_s)=2\,K(a_1,\ldots,2A,\ldots,a_s)K(a_1,\ldots,2A,\ldots,a_s\,A)\,,$$

$$=2^2K(a_1\,\ldots,a_s)\,K(a_1,\ldots,a_s\,A)\,,$$

$$\times\,K(a_1,\ldots,2A,\ldots,a_s\,A)\,,$$

$$K(a_1,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,a_s)$$

$$=2^3K(a_1,\ldots,a_s)K(a_1,\ldots,a_s\,A)$$

$$\times\,K(a_1,\ldots,2A,\ldots,a_s\,A)$$

$$\times\,K(a_1,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,a_s A)\,,$$

$$.\;.\;.\;.\;.\;.\;.\;.\;=\;.\;.\;.\;.\;.\;.\;.\;.$$

So that if we put for shortness' sake

$$K_0=K(a_1,\ldots,a_s)\,;$$

$$K_1=K(a_1,\ldots,a_s\,A)\,,$$

$$K_2=K(a_1,\ldots,a_s\,2A,\,a_1,\ldots,a_s\,A)\,,$$

$$K_3=K(a_1,\ldots,2A,\ldots,2A,\ldots,2A,\ldots,a_s\,A)\,,$$

$$.\;.\;.\;=\;.\;.\;.\;.\;.$$

the identity becomes

$$A + \frac{1}{a_1} + \cdots + \frac{1}{a_s} + \frac{1}{2A} + \cdots$$
$$\quad\ \ast \qquad\qquad\quad \ast$$

$$= A + \frac{K(a_2, \ldots, a_s)}{K_0} + \frac{(-1)^s}{2K_0K_1} - \frac{1}{2^2K_0K_1K_2} - \frac{1}{2^3K_0K_1K_2K} - \cdots$$

The fourth term is then seen to be derivable from the third by merely dividing by $2K_2$, the fifth from the fourth by dividing by $2K_3$, and so on.

In the next place, each of the K's after K_1 is easily calculable from the preceding K. For example,

$$\begin{aligned}
K_2 &= K(a_1, \ldots, a_s, 2A, a_1, \ldots, a_s, A), \\
&= K(a_1, \ldots, a_s, 2A)K(a_1, \ldots, a_s, A) + K(a_1, \ldots a_s)K(a_2, \ldots, a_s, A), \\
&= \left\{2K(a_1, \ldots, a_s, A) - K(a_1, \ldots a_{s-1})\right\}K(a_1, \ldots a_s, A) \\
&\qquad\qquad + K(a_1, \ldots, a_s)K(a_2, \ldots, a_s, A), \\
&= 2K_1^2 - K(a_1, \ldots, a_{s-1})K(a_1, \ldots, a_s, A) \\
&\qquad + K(a_1, \ldots, a_s)K(a_2, \ldots, a_s, A) \\
&= 2K_1^2 + (-1)^s,
\end{aligned}$$

the last step being due to the fact that

$$\frac{K(a_1, \ldots, a_s, A)}{K(a_1, \ldots, a_s)}, \quad \frac{K(a_2, \ldots, a_s, A_1)}{K(a_1, \ldots, a_{s-1})}$$

are really

$$\frac{K(a_1, \ldots, a_1, A)}{K(a_1, \ldots, a_1)}, \quad \frac{K(a_2, \ldots, a_1, A)}{K(a_1, \ldots, a_2)}$$

—that is to say, are successive convergents—on account of $a_1, \ldots, a_s$ being the same when read backwards as when read forwards.*

Similarly

$$K_3 = 2K_2^2 + (-1)^{2s+1}$$
$$K_4 = 2K_3^2 + (-1)^{4s+3}$$
$$\ldots = \ldots.$$

* If this were not the case we should have—

$$\begin{aligned}
&- K(a_1, \ldots, a_{s-1})K(a_1, \ldots, a_s, A) + K(a_1, \ldots, a_s)K(a_2, \ldots, a_s, A) \\
&= - K(a_1, \ldots, a_{s-1})K(a_1, \ldots, a_s, A) + K(a_1, \ldots, a_s, A)K(a_2, \ldots, a_s) + (-1)_s \\
&= \quad K(a_1, \ldots, a_s, A)\left\{K(a_2, \ldots, a_s) - K(a_1, \ldots, a_{s-1})\right\} + (-1)^s.
\end{aligned}$$

As an example of the application of the series to the extraction of the square root, let us find by means of it $\sqrt{300}$ or $10\sqrt{3}$.

Since

$$\sqrt{300} = 17 + \underset{*}{\frac{1}{3}} + \frac{1}{8} + \frac{1}{3} + \underset{*}{\frac{1}{34}} + \cdots,$$

the series in this case is

$$17 + \frac{K(8,3)}{K(3,8,3)} - \frac{1}{2K(3,8,3)K(3,8,3,17)}$$

$$- \frac{1}{2^2K(3,8,3)K(3,8,3,17)K(3,8,3,34,3,8,3,17)}$$

$$- \ldots\ldots\ldots\ldots$$

Now

$$K(8,3) = 25,$$

$$K(3,8,3) = 78,$$

$$2K(3,8,3,17) = 2702,$$

$$2K(3,8,3,34,3,8,3,17) = (2702)^2 - 2$$

$$= 7300802$$

$$\ldots\ldots = \ldots\ldots\ ;$$

so that the series becomes

$$17 + \frac{25}{78} - \frac{1}{78 \cdot 2702} - \frac{1}{78 \cdot 2702 \cdot 7300802} - .$$

Dividing 1 by 78, we obtain

$$\cdot0128205 \quad 128205 \quad 128205 \quad 128205 \quad 12\ldots;$$

Dividing this by 2702, we obtain

$$\cdot0000047 \quad 448233 \quad 976731 \quad 386057 \quad 81\ldots,$$

and dividing this by 7300802, we obtain

$$\cdot0000000 \quad 000006 \quad 499044 \quad 074436 \quad 12\ldots$$

All we have now got to do is to multiply the first of these quotients by $\frac{100}{4}$, add 17, and subtract the sum of the last two quotients.

Doing so, we find the series

$$\left.\begin{array}{llllll} = & 17{\cdot}320512 & 820512 & 820512 & 820512 & 82\ldots \\ - & {\cdot}000004 & 744824 & 047577 & 546049 & 39\ldots \end{array}\right\}$$

$$= \; 17{\cdot}320508 \quad 075688 \quad 772935 \quad 274463 \quad 4\ \ldots\ .$$

This gives us

$$\sqrt{3} = 1{\cdot}732050 \quad 807568 \quad 877293 \quad 527446 \quad 34\ldots,$$

which is correct to the last-written figure, that is to say, to the *twenty-sixth* place.

Note on Cayley's Demonstration of Pascal's Theorem

By Thomas Muir, M.A., LL.D.

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The demonstration referred to is given in the *Cambridge Mathematical Journal*, vol. iv. pp. 18–20. It opens with the enunciation of a lemma to the effect that the intersection of the planes

$$a_1x + b_1y + c_1z = 0, \quad a_2x + b_2y + c_2z = 0,$$

the intersection of

$$a_3x + b_3y + c_3z = 0, \quad a_4x + b_4y + c_4z = 0,$$

and the intersection of

$$a_5x + b_5y + c_5z = 0, \quad a_6x + b_6y + c_6z = 0$$

will be in the same plane if

$$\begin{vmatrix} a_1 & a_2 & a_3 & a_4 & \cdot & \cdot \\ b_1 & b_2 & b_3 & b_4 & \cdot & \cdot \\ c_1 & c_2 & c_3 & c_4 & \cdot & \cdot \\ \cdot & \cdot & a_3 & a_4 & a_5 & a_6 \\ \cdot & \cdot & b_3 & b_4 & b_5 & b_6 \\ \cdot & \cdot & c_3 & c_4 & c_5 & c_6 \end{vmatrix} = 0.$$

Now, first of all, this condition may be obtained in a different form as follows. If $\xi_{12}\eta_{12}\zeta_{12}$ be a point in the first intersection, we have evidently

$$\xi_{12} : \eta_{12} : \zeta_{12} :: |b_1c_2| : |c_1a_2| : |a_1b_2|;$$

and similarly

$$\xi_{34} : \eta_{34} : \zeta_{34} :: |b_3c_4| : |c_3a_4| : |a_3b_4| ,$$

$$\text{and} \quad \xi_{56} : \eta_{56} : \zeta_{56} :: |b_5c_6| : |c_5a_6| : |a_5b_6| .$$

Consequently the condition may be written

$$\begin{vmatrix} |b_1c_2| & |c_1a_2| & |a_1b_2| \\ |b_3c_4| & |c_3a_4| & |a_3b_4| \\ |b_5c_6| & |c_5a_6| & |a_5b_6| \end{vmatrix} = 0 .$$

Multiplying by

$$\begin{vmatrix} a_1 & b_1 & c_1 \\ a_3 & b_3 & c_3 \\ a_4 & b_4 & c_4 \end{vmatrix}$$

we obtain

$$\begin{vmatrix} \cdot & |a_1b_3c_4| & |a_1b_5c_6| \\ |a_3b_1c_2| & \cdot & |a_3b_5c_6| \\ |a_4b_1c_2| & \cdot & |a_4b_5c_6| \end{vmatrix} = 0 ,$$

and on removing from this the multiplier just used, we have the condition in the final form

$$\begin{vmatrix} |a_1b_2c_3| & |a_3b_5c_6| \\ |a_1b_2c_4| & |a_4b_5c_6| \end{vmatrix} = 0 .$$

It is shown to agree with Cayley's by transforming the determinant of the latter into an aggregate of products of complementary minors.*

* Since the *order* of the three points is immaterial, we see that

$$\begin{vmatrix} |a_1b_2c_3| & |a_3b_5c_6| \\ |a_1b_2c_4| & |a_4b_5c_6| \end{vmatrix} = -\begin{vmatrix} |a_1b_2c_5| & |a_5b_3c_4| \\ |a_1b_2c_6| & |a_6b_3c_4| \end{vmatrix} = -\begin{vmatrix} |a_3b_4c_1| & |a_1b_5c_6| \\ |a_3b_4c_2| & |a_2b_5c_6| \end{vmatrix} .$$

It will be found that the second of the three determinants is got by using $|a_1b_5c_6|$ instead of $|a_1b_3c_4|$ for our multiplier and divisor in the foregoing transformation, and the third by using similarly $|a_3b_1c_2|$. This is one proof of their identity. Another consists in pointing out the simple fact, that to establish the identity of the first two is the same as to prove that

$$|a_1b_2c_3| \cdot |a_4b_5c_6| - |a_1b_2c_4| \cdot |a_3b_5c_6| + |a_1b_2c_5| \cdot |a_3b_4c_6| - |a_1b_2c_6| \cdot |a_3b_4c_5| = 0,$$

—a well-known identity of Bezout's, and, curiously enough, Cayley's second lemma in this very paper.

Taking now the six points 1, 2, 3, 4, 5, 6 whose coordinates are $(x_1y_1z_1)$, $(x_2y_2z_2)$, ... , and calling the origin 0, we have as the equations of the planes 012, 023, 034, ...

$$x|y_1z_2| + y|z_1x_2| + z|x_1y_2| = 0\,,$$
$$x|y_2z_3| + y|z_2x_3| + z|x_2y_3| = 0\,,$$
$$\cdots\cdots\cdots\cdots$$

and therefore the condition that the intersection of 012, 045, the intersection of 023, 056, and the intersection of 034, 061 will lie in the same plane is

$$\begin{vmatrix} |y_1z_2| & |y_4z_5| & |y_2z_3| \\ |z_1x_2| & |z_4x_5| & |z_2x_3| \\ |x_1y_2| & |x_4y_5| & |x_2y_3| \end{vmatrix} \cdot \begin{vmatrix} |y_5z_6| & |y_3z_4| & |y_6z_1| \\ |z_5x_6| & |z_3x_4| & |z_6x_1| \\ |x_5y_6| & |x_3y_4| & |x_6y_1| \end{vmatrix}$$
$$- \begin{vmatrix} |y_1z_2| & |y_4z_5| & |y_5z_6| \\ |z_1x_2| & |z_4x_5| & |z_5x_6| \\ |x_1y_2| & |x_4y_5| & |x_5y_6| \end{vmatrix} \cdot \begin{vmatrix} |y_2z_3| & |y_3z_4| & |y_6z_1| \\ |z_2x_3| & |z_3x_4| & |z_6x_1| \\ |x_2y_3| & |x_3y_4| & |x_1y_6| \end{vmatrix} = 0.$$

The four determinants here, however, are simplifiable after the manner of a similar determinant above. They are, in fact, all seen to be of the type

$$\begin{vmatrix} |y_\alpha z_\beta| & |y_\beta z_\gamma| & |y_\delta z_\epsilon| \\ |z_\alpha x_\beta| & |z_\beta x_\gamma| & |z_\delta x_\epsilon| \\ |x_\alpha y_\beta| & |x_\beta y_\gamma| & |x_\delta y_\epsilon| \end{vmatrix},$$

and this multiplied by

$$\begin{vmatrix} x_\alpha & x_\beta & x_\gamma \\ y_\alpha & y_\beta & y_\gamma \\ z_\alpha & z_\beta & z_\gamma \end{vmatrix}$$

produces

$$\begin{vmatrix} \cdot & |x_\alpha y_\beta z_\gamma| & |x_\alpha y_\delta z_\epsilon| \\ \cdot & \cdot & |x_\beta y_\delta z_\epsilon| \\ |x_\alpha y_\beta z_\gamma| & \cdot & |x_\gamma y_\delta z_\epsilon| \end{vmatrix}.$$

$$i.e.,\ |x_\alpha y_\beta z_\gamma|^2 \cdot |x_\beta y_\delta z_\epsilon|\,;$$

so that we are thus furnished with the useful identity

$$\big\| \, |y_\alpha z_\beta| \, , \, |z_\beta x_\gamma| \, , \, |x_\delta y_\epsilon| \, \big\| = |x_\alpha y_\beta z_\delta| \, . \, |x_\beta y_\delta z_\epsilon| \, .$$

Applying this four times, therefore, to our equation of condition, we have at once the surprisingly simple result

$$|x_1y_2z_3|.|x_2y_4z_5|.|x_5y_6z_1|.|x_6y_3z_4| - |x_4y_5z_6|.|x_5y_1z_2|.|x_2y_3z_4|.|x_3y_6z_1| = 0 \, ;$$

or, as Cayley would write it,

$$\overline{123}\,.\,\overline{245}\,.\,\overline{561}\,.\,\overline{634} - \overline{456}\,.\,\overline{512}\,.\,\overline{234}\,.\,\overline{361} = 0 \, ;$$

or, perhaps better still,

$$\begin{aligned} &\overline{123}\,.\,\overline{156}\,.\,\overline{425}\,.\,\overline{436} \\ &- \overline{125}\,.\,\overline{136}\,.\,\overline{423}\,.\,\overline{456} = 0 \, . \end{aligned}$$

Now, if one of the points 1, 2, 3, 4, 5, 6,—say 1,—be viewed as *current*, this is evidently a cone of the second order, with its vertex at 0. And since 1 is associated in both of the terms with 2, 3, 5, 6, both terms will vanish, and the equation therefore be satisfied when for $(x_1y_1z_1)$ we write $(x_2y_2z_2)$ or $(x_3y_3z_3)$ or $(x_5y_5z_5)$ or $(x_6y_6z_6)$. Also it will be satisfied when $(x_4y_4z_4)$ is substituted, because then the two terms are alike and of opposite signs. The theorem in question is thus established.

On comparison of this with Cayley's investigation, it will at once be seen that not only is the resulting equation here much simpler than his, but that it is also greatly superior for the purpose he had in view.

There still remains the question as to the reconcilement of the two processes. Cayley's resulting equation is

$$\begin{aligned} &\overline{145}\,.\,\overline{256}\,.\,\overline{423}\,.\,\overline{361} + \overline{245}\,.\,\overline{123}\,.\,\overline{456}\,.\,\overline{361} \\ &\qquad - \overline{245}\,.\,\overline{123}\,.\,\overline{356}\,.\,\overline{461} - \overline{245}\,.\,\overline{156}\,.\,\overline{423}\,.\,\overline{361} = 0. \end{aligned}$$

Can it be simplified? Observing that the two middle terms have more than one common factor, we write the aggregate of the two in the form

$$\overline{245}\,.\,\overline{123}\,(\overline{456}\,.\,\overline{361} - \overline{356}\,.\,\overline{461}).$$

The bracketed expression is then seen to be equal to

$$\overline{346}\,.\,\overline{516}\,,$$

by reason of the old identity (used also by Cayley in this paper),

$$\overline{356}.\overline{416} - \overline{346}.\overline{516} + \overline{316}.\overline{546} = 0.$$

Consequently, as a first step, the equation in question is shortened to

$$\overline{145}.\overline{256}.\overline{423}.\overline{361} + \overline{245}.\overline{123}.\overline{346}.\overline{516} - \overline{245}.\overline{156}.\overline{423}.\overline{361} = 0.$$

The process of condensation, however, can be continued, and that in two different ways. If we combine in the first instance the last two terms, which are seen to have the common factor $\overline{245}.\overline{516}$, we read the result

$$\overline{145}.\overline{256}.\overline{423}.\overline{361} + \overline{245}.\overline{516}.\overline{143}.\overline{263} = 0,$$

or, say,

$$\left.\begin{array}{r} \overline{145}.\overline{136}.\overline{234}.\overline{256} \\ +\overline{143}.\overline{156}.\overline{245}.\overline{236} \end{array}\right\} = 0: \qquad (A)$$

whereas if we combine the first and last terms, which have the common factor $\overline{423}.\overline{361}$, we obtain

$$\overline{423}\cdot\overline{361}\cdot\overline{125}.\overline{456} + \overline{245}\cdot\overline{123}\cdot\overline{346}\cdot\overline{516} = 0,$$

or, say,

$$\left.\begin{array}{r} \overline{125}.\overline{136}.\overline{423}.\overline{456} \\ -\overline{123}\cdot\overline{156}.\overline{425}.\overline{436} \end{array}\right\} = 0. \qquad (B)$$

Of these equally simple forms (A) and (B), the latter is the one previously obtained, so that the requisite reconcilement is effected.

Since, however, the left-hand side of Cayley's equation has been shown to be reducible either to the left-hand side of (A) or to the left-hand side of (B), a new question arises, viz., How it comes that the two reduced forms are the same, or how it is that

$$\overline{145}.\overline{136}.\overline{234}.\overline{256} + \overline{143}.\overline{156}.\overline{245}.\overline{236}$$
$$- \overline{125}.\overline{136}.\overline{423}.\overline{456} + \overline{123}\cdot\overline{156}\cdot\overline{425}\cdot\overline{436}$$

vanishes identically? One answer is, that the aggregate of the first and third terms being

$$= \overline{136}.\overline{234}\ (\overline{145}.\overline{256} - \overline{125}.\overline{456})$$
$$= \overline{136}.\overline{234}.\overline{165}.\overline{425},$$

and the aggregate of the second and fourth being

$$= \overline{156}.\overline{245}\ (\overline{143}.\overline{236} - \overline{123}.\overline{436})$$
$$= \overline{156}.\overline{245}.\overline{163}.\overline{423},$$

the two aggregates differ only in sign, and consequently their sum is zero.

On the Connections of the Inferior Olivary Body. By Alexander Bruce, M.A., M.D., F.R.C.P.Ed., F.R.S.E. (With Two Plates.)

(Read January 6, 1890.)

The inferior olivary body or nucleus forms the ovoid projection which extends almost the whole length of the medulla oblongata, from the lower margin of the pons Varolii to within a short distance of the level of the decussation of the anterior pyramids. It is separated from the latter by a groove through which emerge the roots of the hypoglossal nerve. On its outer margin it is separated from the line of the roots of the glossopharyngeal and pneumogastric nerves by a shallow depression. Transverse vertical and longitudinal sections of the medulla show the olive to be a highly convoluted sac of grey matter open, at its hilum, towards the mesial plane. (It has two accessory nuclei of smaller size, an internal and a posterior accessory olive, which, as they are really parts of the larger nucleus, do not call for special consideration here.) The fibres of the hypoglossal nerve pass through its substance, but do not become, as was at one time supposed, and as has been recently again affirmed by Vincenzi, connected with the olive. On its median aspect lies the *interolivary stratum* or *fillet.* Anteriorly lies the anterior pyramid, posteriorly the formatio reticularis.

According to the experiments of Bechterew, the inferior olive is concerned in co-ordinating movements necessary for the maintenance of the equilibrium of the body.* One would naturally from this

* Bechterew (*Neurologisches Centralblatt*, Dec. 1, 1882) states the results of section of the inferior olivary bodies to be as follows:—(*a*) Deep section of the olives produce forced movements, mostly rolling round the long axis of the body towards the injured side, and nystagmus with one eye directed upwards and outwards, and the other downwards and inwards. (*b*) Slighter lesions produced movements in a circular or in a forward direction, or caused the animal to throw itself backwards. (*c*) Sometimes forced positions, with strong lateral curvature of the body, were produced. (*d*) There was generally, also, a tendency to fall towards the injured side, or in lesions of both olives unsteadiness of gait, or actual inability to stand or walk.

In estimating the results of these experiments, it must be remembered that it is impossible to divide the olivary bodies without at the same time injuring some of the following structures, viz., the direct cerebellar tract, the arciform fibres of Solly (or the anterior external arcuate fibres of Edinger), the ascending tract of Gowers, or those fibres which pass from the lateral columns of the

fact (as well as from its large size) expect that it should possess an extensive system of fibres connecting it with other parts of the central nervous system, more especially with such parts as have a similar function; but it is a striking illustration of the difficulty of the histological investigation of this region, that until quite recently only one of these connecting systems had been definitely established.

It has long been known that when one half of the cerebellum is congenitally defective, the restiform body of the same side, and the inferior olive of the opposite side, are also absent, the development of the olive being evidently dependent on the opposite hemisphere of the cerebellum. With regard to the other relations of the olive, opinions have been very divergent.

The most generally accepted view is that of Deiters, viz., that the olive is a nodal point or ganglion of interruption, between the posterior columns of the spinal cord (or their nuclei) of one side and the restiform body of the opposite side. Deiters thought that the internal arcuate fibres which originate in the nuclei of the posterior columns (*i.e.*, the clavate nucleus, or nucleus of the column of Gall, and the cuneate nucleus, or nucleus of the column of Burdach) terminate on the convex surface of the olive of the same side; while other fibres, arising from the interior of the same olive, leave it at its hilum, and after crossing the raphé, and then passing partly anterior, partly through and partly posterior to the opposite olive, enter the restiform body. Deiters considered that the fact that the restiform body gradually increases in volume in proportion as the posterior columns diminishes is an evidence of the intimate association of these two structures.

Meynert, while espousing the opinion of Deiters, pointed out that many of the internal arcuate fibres arising from the cuneate and clavate nuclei pass behind the olive of the same side, and reach

spinal cord to the superior olive, and the posterior corpus quadrigeminum. The lesions produced in these various strands may therefore be, at least in part, responsible for the phenomena attributed to the section of the olivary bodies. Ferrier concludes (*Functions of the Brain*, 2nd edit., p. 207) that the views of Deiters and Meynert, with regard to the connection of the olive and the posterior columns, are established by Bechterew's experiments. In view of the possible fallacies in these experiments, and the certain results of embryological investigations, that connection can no longer, in my opinion, be maintained.

the restiform body of the opposite side without entering either olive.

Ross (*Diseases of Nervous System*) and Ferrier (*Functions of the Brain*, 2nd edition) adopt the views of Deiters and Meynert.

The recent researches of Edinger,* Flechsig, Bechterew (with which I fully agree, on the ground of my own observations in my Thesis for the degree of M.D.), show the view of Deiters, with regard to the termination in the olive of those internal arcuate fibres which originate in the nuclei of the posterior columns, to be untenable.

If one examines transverse sections of the medulla oblongata of a human embryo between the ages of six and eight months which have been stained with hæmatoxylin after the method of Weigert, one finds that the fibres from the olive to the restiform body are not yet invested with myeline, while those internal arcuate fibres which form the cuneate and clavate nuclei are fully medullated. An uncomplicated view of their course can thus be obtained; and it is beyond any doubt that none of these fibres terminate in the olive of the same side, that all the fibres of this system which enter the olive merely pass through its substance, and terminate in the interolivary stratum or fillet of the opposite side.

Flechsig (*Plan des menschlichen Gehirns*) was at one time of opinion that the larger part, or two-thirds of the interolivary stratum or fillet, entered the olive; but it would appear that he no longer maintains this view, and though there seems to be some pathological evidence in its favour (see the case of Meyer, *Arch. f. Psych.*, vol. xiii.), the examination of embryonic brains, especially in longitudinal sections, points to the absence of any such connection.

There is as yet no evidence that any part of the fibres of the spinal cord terminate in the olive, though that may well be the case.

In 1885 Bechterew (*Neurol. Centralbl.*, 1885, p. 194) discovered in the brains of infants of the age of one month a tract passing from the olive in a direction upwards towards the brain, which had been regarded erroneously by Stilling as a continuation of the posterior

* Edinger, *Neurol. Centralblatt*, 1885, p. 73. See also Spitzka's paper, *Medical Record*, 1884, vol. xxvi., Nos. 15–18.

columns of the spinal cord, and by Wernicke as a downward continuation of the posterior commissure. This strand (Plate I. fig. 1, *Bt.*), which he calls the "centrale Haubenbahn," appears on the posterior and external aspect of the olive. It gradually increases in size from the middle of the olive upwards. At the lower part of the pons (Plate II. *Bt.*) it lies between the superior olive and the fillet and dorsal to the fibres of the corpus trapezoideum. In the upper part of the pons it lies in the middle of the formatio reticularis. At the level of the anterior corpora quadrigemina it lies immediately external to the posterior longitudinal fasciculus. It then passes behind the red nucleus, and enters the lenticular-nucleus loop, to terminate, according to Flechsig, in the lenticular nucleus.

About nine months ago, while making oblique sections of the medulla and cerebellum of a nine months' embryo, in order to demonstrate the course of the restiform body (the posterior aspect of the section being thus at a higher level than the anterior), I found another tract (Plate I. fig. 2, *a.o.t.*), apparently previously undescribed. (It is possible that this tract is referred to by Bechterew in the paper quoted above.) The strand, which is only indistinctly seen in the ordinary vertical transverse sections, begins, like Bechterew's tract, on the external aspect of the olive. It then bends gradually inwards, backwards, and in an upward direction, forming a compact bundle, till it reaches the inner side of the nucleus of the vii or facial nerve (vii nucl.). Then it turns outwards and slightly backwards, crossing the roots of the facial nerve (vii rad.). As it does so the fibres spread out from each other in a fan-shaped manner, and become less easy to trace. They apparently, however, bend somewhat downwards and enter the external (Deiters' nucleus) of the vestibular root of the auditory nerve.

This connection of the olive with the auditory nerve may serve to throw some light on the special function assigned to the inferior olivary body in maintaining the equilibrium of the body.

EXPLANATION OF PLATES.

Plate I. fig. 1.—*a.p.*, anterior pyramid; *i.o.*, inferior olive; *B.t.*, Bechterew's tract; *V.Asc.*, ascending root of fifth nerve; *c.r.*, corpus restiforme.

Plate I. fig. 2.—*a.p.*, anterior pyramid; *inf. olive*, inferior olivary body; *v.Asc.*, ascending root of fifth nerve; *vii nucl.*, nucleus of facial nerve; *vii*

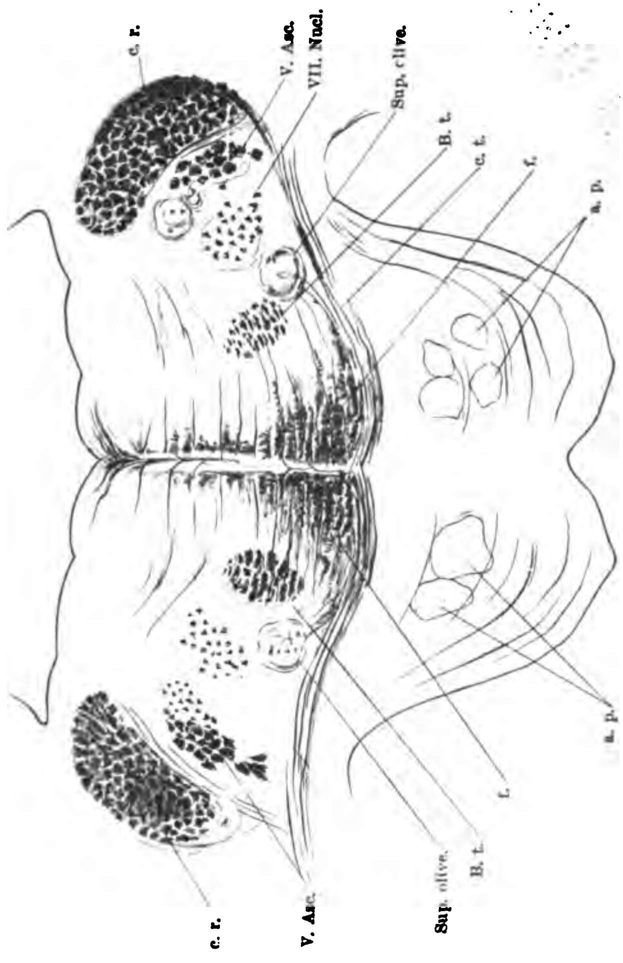


Diagram representing Bechterew's tract in the pons Varolii.

fad., root of facial nerve; *a.o.t.*, acustico-olivary tract; *viii B. nucl.*, Bechterew's nucleus of auditory nerve; *s.p.c.*, superior cerebellar peduncle.

Plate II.—*a.p.*, anterior pyramid; *f.*, fillet; *c.t.*, corpus trapezoideum; *B.t.*, Bechterew's tract; *sup. olive*, superior olive; *vii nucl.*, nucleus of facial nerve; *v. Asc.*, ascending root of fifth; *c.r.*, corpus restiforme.

Enzyme Action in Lower Organisms. By G. E. Cartwright Wood, M.D., B.Sc.

(Read December 16, 1889.)

The soluble ferments or enzymes have always aroused the deepest interest, partly from the mystery which enshrouded their mode of action, partly from the importance of the processes with which they are associated. The peculiar power which each possesses of decomposing apparently unlimited quantities of a specific medium, without itself being used up in the process, has occasioned the confusion of enzyme action with processes truly vital in their nature. Although this action had only been demonstrated as subserving an alimentary function, its aid was invoked to explain many of the more obscure phenomena of biology. The series of decompositions which carbohydrates may undergo, known as the alcoholic, lactic, and butyric fermentations, were long ascribed to it. Even when Pasteur had proved that these processes were always correlated with a vital fact —the growth and multiplication of living cells—Traube,* Hoppe-Seyler, and Liebig † still contended that these might act only indirectly by the formation of soluble ferments. The analogy between fermentation and the infectious processes is so striking that the latter have long been grouped together under the term zymotic diseases, and these we are every day coming to recognise more and more as parasitic diseases conditioned by micro-organisms. Here again, however, many tend to regard the microbe as not acting directly, but through the production of soluble ferments. The consideration of enzyme function in lower organisms has accordingly another interest than that which attaches to it, as throwing light upon the

* *Theorie der Fermentwirkungen*, Berlin, 1858.

† *Ueber Gährung, Quelle der Muskelkraft und Ernährung*, Leipsic, 1870.

processes of digestion in the higher animals. Upon the view which we take as to its origin and meaning will depend the standpoint from which we regard many important physiological and pathological questions.

Although Duclaux and Hueppe, in their investigations on milk, had suggested the probable existence of soluble ferments, H. Bitter in 1887 first furnished rigorous proof that bacteria produce enzymes separable from the organisms which form them. He managed to kill the organisms by sterilisation at 60° C. without materially destroying their products, and in this way demonstrated that two organisms, when grown on gelatine, produced enzymes which were able, apart from the organism, to liquefy gelatine and peptonise albumen. Sirotinin, in an investigation carried out in Flügge's laboratory, noted incidentally, that culture fluids which had been freed from organisms by means of Chamberland's porcelain filter, still retained the power of liquefying the gelatine. Brunton and Macfadyen have, more recently, communicated the same fact for another organism, and have stated in addition that when grown on starch a diastatic ferment was there produced. My own experiments were undertaken, not for the purpose of demonstrating the existence of such enzymes, but if possible to investigate more minutely their properties. For this purpose the four organisms which form what is known as the cholera group—Koch's cholera bacillus, Deneke's cheese bacillus, Finkler's cholera nostras bacillus, and Miller's bacillus—were selected. These organisms exhibit in all directions a most striking similarity, and the point which I wished to investigate was whether their enzymes could be shown to differ in any way. The reaction in which they would continue to act offered itself as the most convenient test. The method of experimentation pursued was as follows:—A tube containing 20 c.c. of sterilised milk was taken, and to it a certain quantity of acetic acid was added. The fluid was shaken, and then delivered, by means of a sterilised pipette, in equal quantities, into four test tubes. To each of these 1 c.c. of the *sterilised* products of one of the organisms was added, and the result noted. By making a series containing varying quantities of acid the effect on the enzyme action was ascertained. The cholera enzyme was found to be most sensitive towards acid reaction, even very small quantities inhibiting its activity. Deneke came next, while

Finkler and Miller bacilli continued to act in distinctly acid solutions. But the two enzymes which each of these organisms possess seemed not to be equally affected by the acidity. The precipitation of the casein occurred in reactions where the peptonisation was apparently completely inhibited. We must therefore conclude that the enzymes of these organisms are, in each case, distinct, and that the enzymes, even in the same organism, are not alike as regards their capacity of acting under different conditions.

Gelatine and blood serum cultures of the four organisms were found, in no case, to exert any diastatic action on starch. A medium containing starch was then inoculated with cholera bacillus, which after being allowed to grow for some time was sterilised at 60° C. The amount of sugar present was then estimated in a small quantity. By means of a pipette, 25 c.c. of the sterilised culture was then introduced into a starch solution, and incubated for six days, when the amount of sugar was again titrated. The quantity found was not, however, greater than what had been introduced with the sterilised culture fluid, so that no evidence of ferment action was exhibited. Although the experiment was repeated twice, a negative result had, on both occasions, to be recorded. It is possible that the ferment may have been destroyed, or that some condition was present which prevented its action. It appears to me more probable, however, that the power of converting the starch into sugar may here exist only in the protoplasm, it not having yet become developed into an enzyme separable from the organism. It is not necessary to consider the primary processes of digestion as dependent on soluble ferments. The enzyme function should, as Hueppe * has insisted, be regarded as a *secondarily* derived function of the protoplasm. The process of digestion, as observed in its simplest form, is an intracellular digestion, and is not, as Krukenberg † has conclusively shown, dependent on enzymes, but is inherent in the protoplasm itself. The enzymes are merely further differentiations of this primitive power of the protoplasm in the same way as muscle and nerve are to be regarded as represented in the contractility and irritability of a structureless mass of undifferentiated protoplasm. We are accordingly prepared

* *Die Methoden der Bakterien-Forschung*, 1888; "Abschnitte ueber Biologie," *Die Hygienische Beurtheilung des Trinkwasser*, 1887.

† *Vergleichend-physiologische Vorträge*, Heidelberg.

to find very different degrees of development in different organisms, and even in the same organism, as regards the different substances to be digested. Thus cholera appears to digest its proteids by means of enzymes, but the faculty of digesting the carbohydrates appears still to reside in the protoplasm. How far the process is carried out in each case by means of enzymes, and how far, directly, by the protoplasm, must be decided by experiment. Even in the alimentary canal the process is not carried out by ferments alone. Starch is there converted into equal quantities of maltose and dextrine, and the latter is unaffected by the further action of the enzymes; yet as neither of these bodies is absorbed as such, the portal vein containing the carbohydrates only in the form of dextrose, these must undergo their further changes at the hands of the cells. In a similar way lactose and cane sugar are split up by an inversive ferment into equal quantities of dextrose and levulose, and the further transformation of the latter is evidently to be referred directly to the protoplasm.* The greater part of the albumen is undoubtedly peptonised before being absorbed, yet, as Voit and Bauer † have shown, a part may undergo direct absorption. More recent experiments, in cases of artificial anus, where the influence of the ferments from the upper straits of the canal was completely excluded, have shown that egg albumen undergoes direct absorption; and since egg albumen is excreted by the kidneys as a foreign body, the disintegration and rebuilding up which is necessary for its assimilation must be attributed purely to cellular action. In all probability the cells lining the alimentary tract perform a much larger part of the preliminary processes of digestion than is at present conceded to them.

An enzyme is accordingly to be looked upon as a function which has undergone a high degree of differentiation, indeed, as a property which is able to exist and act apart from the protoplasm. As each organism is adapted to special conditions, we should expect the enzymes also to act best under these conditions. It has already been stated that the enzymes of cholera—Deneke, Miller, and Finkler—

* Although the enzyme obtained from yeast can convert only the half of the cane sugar into dextrose, Payen has shown that the living yeast-cells convert the levulose also into dextrose.

† *Zeit. für Biol.*, Bd. v. 562.

exhibit a varying susceptibility to acid reaction precisely as the organisms themselves do. This does not indicate that the organisms are more susceptible to acidity, according as their enzymes are more sensitive to its presence, but that the protoplasm as a whole, and with it the enzymes, is adapted to a certain set of conditions—its usual environment.

This correspondence between the conditions under which the organism exists and those under which the enzymes act, is exhibited even more strikingly by a consideration of their temperature relations. Every enzyme has an optimum temperature at which it works most effectively, and a lower and a higher limit at which it ceases to act. In the case of the cold-blooded animals this optimum temperature is much lower than in the warm-blooded animals. Fick * and Murisier† found that the gastric juice of the frog, pike, and trout dissolved albumen at 0° C. Hoppe-Seyler ‡ has confirmed this result, and states in addition that at higher temperatures they did not act so effectively as at moderate temperatures. The gastric juice of a mammal, on the other hand, peptonises best at the temperature of the body, 39° C., and becoming less active at lower temperatures, is finally inoperative at 10° C. Again, while the diastatic ferment of the pancreas and saliva acts best between 37° and 40° C., that of germinating barley, where the temperature is usually much higher, has its optimum at from 54° to 63° C. Micro-organisms exhibit even greater differences as regards the temperature to which they are adapted; some grow and liquefy the gelatine at 0° C., and as is seen in the case of the micrococcus, which causes the phosphorescence at sea, can at this temperature exhibit their characteristic action; § others do not grow at a temperature under 50° C., and at 70° C. continue to vegetate actively.|| In accordance with the adaptation of the organism as regards temperature, the liquefaction of the gelatine, occasioned by a microbe, may vary greatly, even to the point of cessation, with the temperature; but how far this is due to lessened production of the enzyme, and how far to its action being interfered with,

* *Arbeiten Physiol. Lab. Würzburg*, ii. p. 181.

† *Verhandlung. d. phys. med. Ges. in Würzburg*, Bd. iv. p. 120, 1873.

‡ *Pflüger's Archiv*, Bd. xiv. p. 395, 1877.

§ Forster, *Central. f. Bakt.*, 1887, vol. ii. No. 12; Fischer, *Central. f. Bakt.*, 1888, vol. iv. No. 3.

|| Globig, *Zeit. für Hygiene*, vol. iii. p. 295.

has not yet in the case of these organisms been definitely ascertained.

We have just seen that each enzyme has its own most appropriate temperature, and as the organisms vary as regards their natural food, we should expect their enzymes to exhibit great variety as regards their capacity of acting on different media. This is very strikingly indicated by their action on the different sugars. The yeasts as a group are able to attack dextrose, giving rise to what is known as the alcoholic fermentation, and they are also able to act on cane sugar, which is, however, first converted by an inversive ferment into dextrose and levulose. Yet of all the yeasts only three are known which are able to ferment milk sugar, the power of converting it into dextrose not being possessed by the saccharomyces as a group, although it is a property very widely distributed among bacteria. Of still greater interest is the varying manner in which the same organism conducts itself towards different albumenoids. Thus, as a very general rule, those organisms which liquefy gelatine are able to coagulate milk, and then peptonise casein which has been separated, but some organisms which peptonise gelatine are without action on milk. Again, we should expect that those organisms which exhibit no enzyme action on gelatine would be inoperative on milk, and although this is usually the case there are exceptions to this rule. It has been already noted that organisms vary greatly as regards the reaction in which they peptonise the milk, and it would appear that they even peptonise it in different ways. Although the vast majority first coagulate the casein and then dissolve it, certain microbes appear to peptonise the casein directly; this would indicate that the process in the two cases is not identical.* Very striking is the way in which the same organism conducts itself to the different albumenoids, gelatin, fibrin, blood serum, and egg albumen. One organism is unable to liquefy gelatine, but peptonises fibrin; another liquefies the gelatine, but cannot peptonise the egg albumen. They exhibit, in this respect, the utmost diversity, each having its own special idiosyncrasies. We have thus seen, in the course of our investigation, that the enzymes vary in every direction as regards the temperature and reaction in which they act, and the nature of the medium on which they act. They must, accord-

* Caneva, *Ueber die Schweineseuchen.*

ingly, be regarded as specific in their nature, depending on the specific nature of the protoplasm of which they are merely further differentiations.

The question now arises, Are the products originating from the action of different enzymes the same? We know, already, that the sugars and peptones, in certain cases, are different, and more exact investigation, coupled with more delicate methods, will, without doubt, in the future, add greatly to the number of these. We should not, then, regard the peptonisation as merely a breaking down of the complex proteid molecule into smaller, more diffusible molecules, which are in this way more easily absorbed. It is also a disintegration into simpler bodies of probably a quite definite composition, which the protoplasm can either convert into colloid catalysable material to be stored up in the organism, or build up into its own proper substance. The protoplasm of bacteria has been shown to exhibit the greatest possible difference towards bodies all but indistinguishable from a chemical point of view. We possess indeed in these organisms one of the most delicate reagents at our disposal in differentiating organic bodies of allied constitution. Thus, to take one example out of many, it has been found that the natural and artificially prepared inulins are treated as distinct bodies by a microbe, the first being assimilated, the latter being left absolutely untouched. It is probable, then, that the process of peptonisation, in which the proteid molecule is, as it were, taken to pieces, is not always the same, but varies according to the specific nature of the protoplasm, in which it may have, afterwards, to undergo a process of building up to form an integral part.

The enzymes have not yet been isolated as chemically pure bodies; indeed, there is good reason to believe that, in most cases, these would be found to consist of several bodies corresponding to different stages of their action. This has been indicated most clearly in the case of the diastatic ferment obtained from malt.* The starch, after undergoing a first change into a soluble form, is converted into a molecule of maltose and dextrine; the latter then undergoes successive hydrations, until 52 per cent. of the maltose which the starch could have yielded, has been formed, at which stage the action of the ferment ceases. If, however, the ferment had been

* *Annales de l'Institut Pasteur*, 1887.

previously heated up to 60° C., the starch is converted completely into dextrine as rapidly as previously, but only 28·4 per cent. of the possible maltose producible from the starch is formed, and this quantity is not increased by the addition of more of the weakened ferment. It would appear, from these and other experiments, that the successive stages in the conversion of the starch into maltose depend on different ferments, which are destroyed, perhaps through coagulation, at different temperatures. Little is known of the products which intervene between albumen and peptone, and nothing of the action which the various secretions exert upon them. Yet a complete knowledge of these, and of the action upon them of the various secretions which are poured into the alimentary canal, is essential to a proper understanding of the process. At present the action of a ferment is always tested on the unchanged albumen, usually egg albumen or fibrin, although, as is probably the case with the intestinal secretion, it may operate only in the later stages in the splitting up of the albumen. This may perhaps account for the want of action of this secretion upon the chief articles of food which so many observers have recorded. The products resulting from the action of the secretions on the different forms of albumen in raw and in cooked condition,—the nature and relative amount of the peptone and bye-products formed in each case,—still require more exact investigation. By a consideration of the relative amount of peptone yielded by each we should be able to form a much more accurate conception of its dietetic value. The nature and amount of the bye-products, and the effect, in this relation, of prolonged action of a secretion, as may frequently occur in gastric digestion, might be expected to throw much light on the effect of diet in such disorders of assimilation as gout.

The enzyme function is, as we have seen, to be regarded as a higher development of a primordial function of the protoplasm, and we have now to consider the question as to whether the degree of development in each organism is a fixed invariable quantity or one subject to variation. It follows, from what has been already stated, that such external conditions as temperature, nature, and reaction of the medium affect its action; we have still to mention here that Flügge * has observed that organisms when grown without the

* *Die Mikro-Organismen*, page 470, 1886.

presence of oxygen—under conditions of *anaërobiosis*—appear then to lose their enzyme function, inasmuch as they then cease to liquefy the gelatine. As great inconvenience results in many investigations from the liquefying colonies destroying the gelatine plates before the other, more slowly-growing organisms, have had time to develop, Chantemesse and Widal * have attempted, by the addition of some substance to the medium, to hold the peptonising power temporarily in check. To facilitate the discovery of typhoid fever bacilli in the presence of other organisms, as in suspected earth and water, they recommended the addition of carbolic acid to the gelatine. Control experiments carried out by me at the request of Professor Hueppe,† indicated that the quantity required to effect this in different species of bacteria varied from 0·01 per cent. to 0·1 per cent. carbolic acid, and that the higher concentrations necessary for the more refractory, entirely prevented the development of more sensitive organisms. Other substances were accordingly investigated for the purpose of selecting one which, while retarding the liquefaction of the gelatine, would still exert no injurious action on the microbes. Of those experimented with, glycerine seemed most suitable for general use, although the influence which it exerted upon this function varied greatly in different organisms. This is explained by the fact, that its action appears to consist not in operating directly upon the enzymes, but rather in offering the microbe a substance as pabulum, which it selects in preference to the gelatine. It has been already mentioned that Brunton and Macfadyen found that an organism produced a peptonising ferment when grown on albumen, but a diastatic ferment on starch, so that these would appear to be produced according to the special temporary requirements of the organism. I had previously observed that all the members of the cholera group, when inoculated on blood serum, produced sulphuretted hydrogen, and at the same time liquefied the medium. If, however, glycerine is added to the blood serum the liquefaction and production of sulphuretted hydrogen appear much later, which is, I think, to be attributed to the organism obtaining the energy which it required, first from the more easily decomposed carbohydrate, and, only when this had

* *Gazette Hebdomadaire*, 1887, page 146.

† *Die Methoden der Bakterien-Forschung*, page 299, 1888.

become exhausted, resorting to the albumenoids present. The degree of retardation in the liquefaction of the gelatine will in each case depend on the protoplasmic idiosyncrasies of the organism as regards the form of nourishment best adapted to its metabolism. It is not improbable that an analogous selective action of the protoplasm may come into play when organisms are grown *anaërobically* on gelatine containing grape sugar, and that the absence of liquefaction in some of these cases is to be ascribed to this.

It is not, however, such temporary abeyances of function that I wish to consider now, but more permanent changes in the organisms themselves as regards this function. The first occasion on which I observed the complete loss of the power of liquefying the gelatine was in the case of an old gelatine culture of Koch's cholera bacillus. The colonies originating from it presented the usual irregular margin and granulated-glass appearance, and, under the microscope, cover-glass preparations gave the characteristic comma form, but it grew in gelatine without any signs of liquefaction, and in milk no separation of the casein took place. On potatoes, which are naturally of acid reaction, the growth was either much impaired or even completely inhibited, although other samples of cholera grew luxuriantly on slices of the same potato. This sensitiveness of the organism to acid reaction, when it has lost its enzyme function, will be dealt with later. The variety of cholera which had lost its enzyme faculty, preserved its new properties for many months when cultivated on agar-agar. When grown under other conditions, it rapidly regained its power of liquefying the gelatine. Thus, if frequently re-inoculated on fresh bouillon, and retained at a temperature of 25° C., it recovered its former characters in about three weeks. Since noting this loss of function in the case of cholera, I have had occasion repeatedly to observe the same fact in old cultures of many other organisms. Gelatine plates from such tubes presented non-liquefying colonies often intermingled with the characteristic liquefying ones, which had alone been expected. At first sight it would appear that the former must be impurities, a view which, by preventing further investigation, has no doubt misled many who must at some time have observed the same appearances. I was able, in such cases, by cultivation under appropriate conditions, to cause such aberrant forms to recover their original characters.

The question now arises, What are the factors which come into operation in old cholera cultures, and cause this loss of function? It has been already stated, that when oxygen is excluded, the organism exists without the aid of enzymes. Now, if a culture is left, for long, undisturbed, a membrane forms on the surface which effectually prevents the entrance of oxygen to organisms which have fallen to the bottom of the tube, and the habit of taking its food otherwise than by enzymes may perhaps persist for some time after it is again grown under ordinary conditions. The most important factor, as evidenced by the fact that the age of the culture is essential, is undoubtedly the action of the metabolic products of the microbe, which accumulate under such circumstances. These products may act either as general depressants devitalisingly upon the protoplasm as a whole, or specifically upon one or other function. We must in this case attribute the action to the presence of some special substance, which acts upon this function, as organisms which were unaffected by their own products suffered this loss when cultivated in sterilised cholera cultures. As the phenols had been found to act powerfully in restraining the action of the soluble ferments, attention was directed to indol, which Brieger and Salkowski had shown to be formed by cholera. The great difficulty and expense involved in the preparation of pure indol prevented the experiment from being carried out with it, so recourse was had to phenol. The method of procedure was as follows:—A series of tubes each containing 10 c.c. of sterilised bouillon, received quantities, varying from 1 to 10 drops, of 5 per cent. carbolic acid solution. The organism was then seeded in these tubes, and the effect of the varying concentrations investigated. The microbes which were experimented upon in this way were Koch's *Cholera bacillus*, *Micrococcus indicus*, *Micrococcus prodigiosus*, and *Bacillus pyocyaneus*. These varied greatly as regards the quantity required to effect the loss of their enzyme function, 1 to 2 drops being sufficient for cholera, while *Prodigiosus* and *Indicus* required the highest concentrations. *Bacillus pyocyaneus* being still more intractable, showing itself unaffected, even by 10 drops of carbolic acid, it was finally omitted from the series. The state of the enzyme function in the other organisms was tested from time to time by inoculation on milk and gelatine. By this means, in the space of about six weeks, varieties of cholera, *Prodigiosus*

and *Indicus*, were produced which grew on gelatine without any signs of liquefaction. The condition was not, however, a very permanent one in cholera, as after a certain time a slow liquefaction of the gelatine began to show itself.

In the case of all the organisms, the longer they were subjected to the influence of carbolic acid the more fixed was the new condition. On the other hand, the strongest solutions in which the organism could be got to grow were not those best suited for the production of more permanent species. No doubt, the enzymes were lost more quickly by the use of the higher concentrations, but on recultivation on other media they almost immediately returned. It would appear that the carbolic acid, by acting devitalisingly on the protoplasm as a whole, restricted the exercise of that lower function, which by replacing its enzyme action, prevented the tendency to relapse reasserting itself, when cultivated again under normal conditions. At any rate, it was the use of medium concentrations over more prolonged periods which were found most effective, and indications were not wanting that a previous growth of the microbe in the more dilute solutions would have produced even better results. The enzyme which acted on the gelatine, and that which occasioned the rennet-like separation of the casein in milk, were not affected alike in these experiments. In all three organisms the power of coagulating the milk appeared to be lost before the power of liquefying the gelatine. If this were so, one might consider the rennet-like ferment as being a later evolved function, and so more easily lost when subjected to degrading influences. For, as the precipitation of the casein is only a splitting, preliminary to the peptonisation which is to follow, such an enzyme would be of no use to the organism unless the peptonising power were also present. The absolute proof that it is first lost, is however wanting, as another factor must here be taken into consideration. These organisms become more sensitive to acid reaction in proportion to the loss of their enzyme action, as is evidenced by their less vigorous growth on potato. Now, as they all produce acid from the sugar in milk, their further enzyme production may be inhibited by the acid they themselves have formed before such ferment is present in sufficient quantity to cause the precipitation of the casein.

It may be mentioned here that *Prodigiosus* and *Indicus*, which

had lost the power of liquefying the gelatine had also lost the faculty of forming their characteristic pigments.* This function was, however, much more easily lost than the enzyme function, for perfectly colourless cultures could be obtained, which nevertheless acted on the gelatine and milk, and it was evident that by properly graduating the agent brought to bear on the organism, the one might be lost without the other being appreciably affected.

It has already been stated that Flügge found that micro-organisms when grown in the absence of oxygen, appeared to obtain their food without the aid of enzymes, as they then ceased to liquefy the gelatine. The most natural supposition is, that oxygen being necessary for the formation of the enzymes, when it is excluded, the protoplasm reverts more or less to the primary form of digestion—that without enzymes. This may, in some cases, be the explanation, but there are a number of facts which tend to show that in some cases under *anaërobiosis* the products of the organism are different from those which occur under *aërobiosis.* This has led Hueppe and Brieger to suggest that the metabolism of the organism under *aërobiotic* and *anaërobiotic* states may be different, these two being entirely different adaptations of the protoplasm. On this view, under *anaërobiosis*, they do not liquefy the gelatine, because they then take their food in a different way from what they do under *aërobiosis.* If this conception should, at first sight, appear rather fantastic, I must refer to certain facts which indicate that such organisms may have an adaptation to even so impalpable an agent as light. Prove † found that *Streptococcus ochroleukus* produced its yellow pigment only when exposed to diffuse daylight, while Scholl states that *Bacterium mycoides roseum* formed its red pigment in darkness only. There is no escape, here, from the conclusion that these two organisms have developed their pigment function under different conditions, which alone permit of its exercise, a result in perfect harmony with the beautiful researches of Engel-

* *Staphylococcus aureus*, when obtained from an abscess, must frequently be cultivated for some time before the pigment faculty returns ; other organisms when exposed to the action of the tissues exhibit a temporary loss of the enzyme function. In both cases this may be attributed, in part to an adaptation to the conditions present in the animal organism, and in part to the attenuating influence exerted by the cells.

† Cohn's *Beiträge zur Biol. d. Pflanzen*, Bd. iv., 1887.

mann,* on the influence of light on the *Purpurin bacteria.* We may, accordingly, regard the protoplasm as having several adaptations, latent memories ready to spring into action on the application of the appropriate stimulus.

As we have already seen, the enzyme faculty is to be regarded as a differentiation of a primitive power of the protoplasm, and we have now to consider the probable cause of its origin. The resolution of the complex into the simpler, more diffusible molecules, is accompanied by the setting free of a certain amount of energy which is lost to the organism when this occurs by means of enzymes in the external medium. What compensating advantage could its development entail on the organism? From experiments on *anaërobiosis*, I was led to suggest some time ago, that under this condition, where the enzyme action appears cut off, the organism was more sensitive to acid reaction. Further investigation has indicated so clearly a relation between the loss of this function and a corresponding increase in sensitiveness of the organism to injurious agencies, that this view becomes more than a mere hypothesis. The development of enzymes, by which the complex indiffusible compounds which supply it with nourishment are rendered diffusible outside the organism, would allow of the protoplasm being invested by a firmer, resisting, bounding membrane; and as this function was gradually evolved, each stage of development would be associated with a corresponding condition of the cell membrane. In a similar way each loss of this function would be accompanied by a change in the membrane, rendering it more sensitive to external injurious agencies. We must now consider how far this accords with a series of facts which have lately been observed by Flügge.

Flügge † has come to the conclusion that Pasteur's anthrax and fowl-cholera vaccines are really degenerated cultures, in which the protoplasm as a whole has suffered by the devitalising conditions to which they have been exposed during their production. He founds this view on a series of very careful experiments, which indicated that the attenuated cultures grew less rapidly than the virulent, and were also much more sensitive towards antiseptic agents. In addition, they grew more slowly and were more sensitive

* *Archiv f. d. ges. Physiologie,* Bd. xlii., 1888.
† *Zeitschrift für Hygiene,* Bd. iv., 1888.

to external injurious agencies in proportion to the degree of attenuation they had undergone. The incapacity of the vaccines to grow and cause disease in the bodies of susceptible animals he ascribed to their lessened reproductive power and greater sensitiveness, which gave the victory to the animal in the struggle for mastery. He saw no reason to suppose that the action of these microbes depended in any degree on the production of specific toxines. As his experiments were carried out in greatest detail on anthrax, I will limit myself at present to its consideration. On comparing the action of the virulent organism and its vaccines on Koch's gelatine, the vaccines were seen to liquefy the gelatine much more slowly, and had in fact lost in great part their enzyme function. This effect was much more clearly shown in milk. The normal anthrax coagulated the milk on the second day, while Pasteur's 1st *vaccine* only began to show signs of this after, at earliest, two weeks. My suggestion that the phenomena were in great part to be accounted for by interference with enzyme function was confirmed by this result. The method by which Flügge compared the resistance of the organisms to acids was, shortly, as follows:—A series of test tubes, each containing the same quantity of gelatine, was prepared, and to each set a given number of drops of the acid to be tested was added. The organisms were then inoculated by means of a platinum wire in a set of tubes containing the same quantity of acid, and their growth compared. I was able, however, to register more exactly the effect of the acidity by the following modification. The acid gelatine was rendered fluid, and after being inoculated was, as it cooled down, distributed uniformly on the walls of the tube. By this means the organism was enveloped on all sides by the medium whose influence was to be tested. In addition, as the organisms became separated from one another in the liquefied gelatine, the colonies which appeared later would each originate approximately from one organism, and thus the disturbing influence arising from the varying quantity introduced in each case was eliminated. I was, by this method, able to confirm Flügge's statement, that the *vaccines* were much more easily inhibited in their growth than the unaffected organism. A new tube was now added to each set, and inoculated with virulent anthrax which had been grown for some time *anaërobiotically.* The *anaërobe* organism was found to be more sensitive than the *vaccines,*

so that we must conclude that the loss of virulence and susceptibility to antiseptic agents have no necessary connection. The susceptibility was in all probability to be referred to the change in the cell membrane induced by the loss of the enzyme function. The pigment microbes which had lost their power of liquefying the gelatine were also compared with the unaffected organisms, and found to have become much less resistant to acidity.

It has been already stated that if the application of the degrading agency is properly graduated, *Prodigiosus* and *Indicus* can lose their pigment faculty, although their power of liquefying the gelatine is not appreciably lessened. It is probable that in a similar way anthrax can lose its toxine faculty without its other functions being seriously affected. In support of this it may be mentioned, that Gamaleia states that his *vaccines*, prepared by means of bichromate of potash, did not exhibit the great loss of vitality which Flügge observed in Pasteur's *vaccines*. Pasteur's method of attenuation by the use of high temperatures would appear to be so coarse a method that not merely the pathogenic function, but the vitality of the organism as a whole is affected. But this general loss of function and lessened vitality, depending as it does on the method employed, can in no sense be regarded as characteristic of the *vaccines*.

We have had occasion, frequently, in the course of this paper to refer changes in the power of resistance of an organism to a probable change in the condition of the cell membrane, and it may be not uninteresting here to briefly mention certain morphological variations in the bacillus, some of which may, perhaps, be regarded from this point of view. I have repeatedly observed that when virulent anthrax has been grown for some time in bouillon, *anaërobically*, the shape of the organism undergoes a very remarkable change. The rods and filaments are no longer to be seen, and in their place we have a series of larger spherical or oval bodies, apparently multiplying by fission to form chains and clusters. This torula-form is best investigated in its natural state, as it does not stain readily and appears to shrivel up when dried. When examined in this way the appearance of these spherical bodies is so different from what one finds normally in anthrax, that one feels inclined to put down its presence to the entrance of some chance impurity. Gelatine plates laid down from such cultures proved however that the cultivations

consisted of pure anthrax, and inoculation of mice indicated that their pathogenicity was unimpaired. It is but right to state here that Pasteur * has observed precisely similar phenomena in the case of certain moulds and mucors. These fungi, when grown with exclusion of the air, tend to produce, instead of the usual *mycelium* and *hyphæ*, cells much larger than normal and more globular in form, so much so that Pasteur would refer in part to this, the erroneous statements so many botanists have made concerning the transformation of these into yeast. The *anaërobe* cells of these fungi suggest by their appearance a tendency more or less pronounced to revert to the amœboid state. Another appearance which may be noted in anthrax cultures grown *anaërobiotically* also indicates a change in the cell membrane. Under normal conditions this organism never forms on its cultivation fluids anything approaching to a surface membrane, at most only a narrow ring of growth appearing round the walls of the tube, which falls to the bottom as soon as it has attained any size; but when the air is excluded the superficial growth remains attached until almost the whole surface is covered. Now a surface membrane or zooglœa results, as does the matrix of cartilage, from the changes which the cell membranes undergo in swelling up and yet remaining in continuity, and a change in this respect is what we might expect when grown in this way. The *vaccines* under certain conditions exhibit an appearance, probably of a somewhat similar character, which points in the same direction. When virulent anthrax is grown *aërobically* in bouillon, the fluid remains clear and limpid, the growth collecting at the bottom as a whitish fluffy mass. The *vaccines*, if allowed to grow slowly, present a precisely similar appearance; but if placed at a higher temperature, where they grow more rapidly, the tubes of bouillon, especially in the case of the most attenuated, present a uniformly turbid aspect. The apparent change in the specific gravity of the organism, which permits it to remain in suspension in the fluid, is evidently to be ascribed to the condition of the membrane in the *vaccine* being still further accentuated by the rapid division of the cells at the higher temperature. The *vaccines* are described as morphologically indistinguishable from the virulent organism; Gamaleia,† however, asserts,

* *Études sur la bière*, 1876.
† *Annales de l'Institut Pasteur*, 1888.

in opposition to other observers, that the bacilli of the *vaccines* are smaller, this corresponding to the degree of attenuation they have undergone. On comparing the bacilli of Pasteur's *vaccines* with the virulent forms when grown on gelatine and bouillon, only a very slight difference in their size was usually to be detected; when, however, they originated from a potato culture, the difference became very marked, the 1*st vaccine* being only about one-half as broad as the virulent organism. This appears to be mainly due to the acid reaction of the medium affecting the *vaccines* more than the virulent organism, the size of the bacillus apparently varying with the more or less favourable conditions to which it is for the moment exposed. Huber * has observed that anthrax bacilli vary in size as they occur in the tissues of different species of animals, being much larger in the more sensitive than in the less susceptible animals. This difference in size of the organism in different animals, or even in different tissues of the same animal, should not be regarded as indicating a more or less favourable medium in a physical sense, but rather as the vital expression on the part of the microbe of the influences to which it has been there exposed, resulting from the reactive powers of the tissues in that special animal or even tissue. This point will be dealt with more fully presently, since it is important as evidencing an action exerted by the cells on the microbe which lies outside them.

There is still a relation of the enzyme function to the organism which has not yet been discussed—the action, or rather want of action, which the ferments manifest on the living protoplasm. The organisms are constantly bathed in their own enzymes, and may even be exposed to those of other microbes without their substance being attacked by these. The explanation of Pavy,† of why self-digestion of the stomach does not occur in the living animal, even if it were sufficient in that special case, would not apply to those ferments which act in alkaline reaction. The "living principle" of John Hunter,‡ as inhibiting the action of the enzymes, seems still the only plausible explanation which applies to all cases. This question has assumed importance from a bacteriological point of

* *Deutsche med. Wochenschrift*, 1881.
† *Philosoph. Transactions*, page 161, 1863.
‡ *Philosoph. Transactions*, June 18, 1772.

view, since Metschnikoff has ascribed the death of microbes in the animal body to the mesoderm cells which are able to take up the organisms as foreign bodies and *digest* them. Now, as the organisms appear to be unaffected by enzymes,* by general consent the antiseptic action of the gastric juice being ascribed entirely to its acidity, even assuming that they actually undergo their death in the cells, we have no reason to believe that this is due to a process of digestion, but rather to some influence exerted by the living protoplasm with which it must there come into such close relations. It is probable that the struggle which may occur between two microbes in our test tube cultivations does not differ essentially in *nature* but only in degree from that which takes place in our body between the tissues and the disease germ. In both cases the ultimate result of the action and reaction of the cells upon each other may turn on the sum of the "conditions of existence" to which they are exposed, happening to favour one more than the other. Thus when two organisms are sown together in a culture fluid, which will "overgrow" the other may depend on the relative quantity of the two primarily introduced, the nature and reaction of the medium, and the temperature at which they are held: in a precisely analogous fashion, the growth of a disease germ introduced into a susceptible animal and the disease which follows is dependent on the quantity introduced (Davaine, Watson-Cheyne), the tissue inoculated (Koch, Watson-Cheyne), and it may be the temperature at which the animal is kept (Gibier, Metschnikoff, Pasteur). It was for long supposed that the products of one organism might be highly injurious to another, and that thus the sudden disappearance of an organism introduced into a mixture might be explained, but more recent experiments tend to ascribe much less influence to the products as direct poisons. Kitasato has

* Organic matter in nature is, in the process known as putrefaction, resolved by microbes into its simpler constituents, which in the protoplasm of plants are again built up into complex organic bodies. But a part of the organic matter undergoing this process is built up into the substance of the microbes. These, when dead, appear to undergo a process of disintegration, to be ascribed in all probability to the action of their own enzymes, the cellulose ferment which Vignal has found to be secreted by "potato-bacilli" exerting its action on the cell wall, and the peptonising enzyme upon the albuminoid constituents. In this way the constant circulation of organic matter between the animal and vegetable kingdoms may proceed without intermission.

recently reported that he found that the organisms which, when together, destroyed cholera, did not exert nearly the same effect when seeded as pure cultures alone with cholera, a result which he seemed to regard as very anomalous. This, however, becomes easily understood if we consider the course of events in putrefaction as it occurs normally in nature. If we examine from time to time a fluid undergoing this change, we find that at different periods of the process different organisms predominate, the others, for the time, sinking into the back-ground or even disappearing; yet, as Hauser has shown, several of these organisms can, when grown alone as pure cultures, carry out, although much more slowly, the greater part of the process. In nature we have, however, a physiological division of labour, each organism becoming adapted to a special stage of the process, during which by its more active growth and the molecular changes it sets up in the medium, it seems to exert a certain inhibitive action on the other microbes present. This is more clearly shown by the remarkable way in which yeast * remains essentially pure when grown under favourable conditions, although exposed to every chance impurity. If the wort is held at a suitable temperature, and sufficient yeast is at the first added, so that it may increase sufficiently rapidly to get at an early period "within striking distance" of any chance intruders, no other organism appears able to obtain a footing, unless when the process is allowed to proceed so far that the products of the yeast begin to act depressingly on its own cells. The inhibitory action which one microbe appears to exert upon another during the vigorous exercise of its vital functions, is strictly comparable to that which the tissues may exert upon such organisms. The cells of the human body have undergone great differentiation morphologically and physiologically, fitting them to carry out the various stages in the *dissociations* and *oxidations* necessary in the combustion of albumen to urea, carbonic acid, and water. A microbe on entering the tissues finds itself in conflict with cells specially adapted to the conditions present there, and more or less vigorously carrying out their stages in the vital processes, so that it has small chance of invading the organism, or even holding its ground, unless it exerts a depressing influence by means of its products—unless it is pathogenic. That we are not here ascribing too

* Naegeli, *Theorie der Gährung*, München, 1879.

important a rôle to the *toxines* is indicated by the varying grades of virulence of the organism which at present may be said almost certainly to depend on the production of varying quantities of the poison, and by the fact that when a *tolerance* of the specific poison has been acquired either by a passing attack or by the previous introduction of the toxine, the disease germ loses its power of invading that animal. The microbe under such circumstances need not immediately perish, but may, and probably usually does, undergo a gradual process of attenuation from the increased reactive powers of the tissues, and then falls a victim to the "*phagocytes*" as does an ordinary saprophyte, or even a foreign body. This has been shown by Hueppe and myself * to be the case when animals have been rendered only partially immune to anthrax, the course of the disease is then much prolonged, and when death does occur the microbe is found to have become attenuated.† An Italian observer ‡ has rendered it probable that in a precisely similar way anthrax, when grown with another organism, whose products exerted no injurious action on it, undergoes a slow attenuation, an effect which we must ascribe apparently to the direct action of the one cell on the other. There is accordingly, as already asserted, no reason to assume that any other forces come into play in the destruction of disease germs in the animal body than those we see operating in our cultures.

From a review of what has been said on the subject of enzymes, it is evident that very much less value attaches to the power of liquefying the gelatine, as indicating a fundamental distinction between two organisms, than that which is usually accorded to it. It is a property which is liable to great fluctuations in the degree of its development, each organism in this respect exhibiting specific idiosyncrasies; and although the normal degree of development usually tends to return under cultivation, yet, as in the case of the anthrax vaccines, the lower grade may be retained with great

* "Saprophytismus und Parasitismus," *Berliner klin. Woch.*, No. 13, 1889.

† Since then, it has been shown by Woodhead and myself (*Comptes Rendus*, Dec. 23, 1889) that, if the toxine is antagonised, the tissues are able to come into action, and we have either complete recovery, or the disease is mitigated or prolonged in its course; the microbes on the death of the animal being then found to have undergone a process of attenuation by the action of the cells.

‡ "Sur la concurrence vitale des bacilles la fièvre Typhoide et du bacille Charbon," *Giorn. Internaz.*, ix., 1887.

tenacity. Hueppe, from the mere fact of its being a *secondarily* derived function, considers it of little value for classification, as all characters for this purpose should depend on primary powers of the protoplasm, which as such, are unchangeable. At the same time, the extreme convenience and value of the gelatine methods as a means of diagnosis cannot be overrated, although the result arrived at in this way should in every case be further tested by the appearance of the growth on potato and in milk.

The varying power of resistance of the same microbe, which we have had so frequently to refer to, has a great practical interest in its relation to the most important application of bacteriology—the antiseptic system. Koch proposed anthrax as a standard by which we might compare the relative efficiency of different disinfecting agents. Great confusion has, however, arisen from this, as not only anthrax, but other organisms also, exhibit varying powers of resistance. In determining the absolute value of an antiseptic agent, those conditions which cause the powers of resistance of the organism to vary must be most carefully considered. This is still more necessary at present in consequence of the search after *specific* * disinfectants. It has been found that each organism exhibits idiosyncrasies in relation to antiseptic agents, being exceptionally susceptible to one, and more than usually resistant to another. The object being to find that substance, or combination of substances, which acts most vigorously on the disease germ, and yet injures least of all the tissues, the confusion occasioned by the organism itself not being constant in its properties, can be readily understood. In such experiments even a difference in the age of the culture may cause a serious discrepancy in the results. Thus, if the organisms experimented with are a brood of very young cholera cells obtained by incubation for 18 to 24 hours at the temperature of the body, the action of the antiseptic is much more marked than on an older culture. This is probably to be ascribed to the organisms in this stage of development possessing a more permeable membrane. The nature of the medium appears also to exert a certain influence, as anthrax which has been grown on potato is slightly less resistant than that form other strata, as agar-agar. We have here again to consider the influence which the acidity of the medium exerts on

* Hueppe, *Berliner klin. Woch.*, Nos. 46 and 47, 1889.

the state of development of the enzyme function, a factor which must be constantly borne in mind in all such investigations. In each organism the range of variability, as regards its enzyme action and varying susceptibility, is a specific quantity depending on the nature of the protoplasm, and can only be determined by direct experiment.

The greater sensitiveness of organisms when grown with exclusion of the air may be of more general interest, as perhaps throwing light on a question which has long remained obscure—the manner in which certain diseases are propagated. In cholera, typhoid, and yellow fever, the intestinal symptoms predominate, and it is through the evacuations that the poison is disseminated. Yet although the organisms are present in large numbers, in the case at least of the first two diseases, direct infection from the fresh stools is by almost general consent considered the exception rather than the rule. Pettenkofer has emphasised and attempted to explain this view, especially in the case of cholera, by assuming that the organisms do not leave the intestine in a condition capable of infecting others, but that they must first undergo a process of "ripening" in the soil. It has been suggested by others that the organisms must first grow outside the body to form spores, and that these may be the only means of infection. This is, however, completely negatived by direct bacteriological investigation. Now, the mode of existence of organisms in the intestine must be from the first practically an *anaërobiotic* one. The small quantities of oxygen which are swallowed with the food are rapidly absorbed by the walls of the stomach, or converted into carbonic acid by the organisms always found there, so that, in the upper straits of the small intestine, at most, only traces of oxygen can be present. Experiments carried out by Hueppe and myself, have shown that cholera can under precisely these conditions produce its poison in great quantity. But if the organism lives *anaërobiotically* in the intestine, it will leave it in a peculiarly sensitive condition, especially as regards acids. This greater susceptibility to acids would raise a barrier to its passage through the stomach, through which infection must occur. But, if allowed to grow in contact with the air, as on soiled linen, they would acquire their normal power of resistance, and with it that fearful infectiveness which has been, under such cir-

cumstances, so often remarked. The need for that stage of growth outside the body and the influence of the "time" and "place" disposition as affecting this becomes thus more readily understood. It is not denied that many other factors, such as personal predisposition, and all conditions local or seasonal which favour a state of intestinal irritation, come also into play; but to discuss these at present would be obviously out of place. It is sufficient to have noted that we have here a factor in this question which has not as yet received consideration.

The whole question of enzyme action is of profound interest, from a general physiological point of view. Burdon Sanderson has recently advised us to study function, not in its simplest, but in its most specialised form. Contractility is not to be investigated in a mass of undifferentiated protoplasm, but in striped muscle. We have in the case of the enzymes a function of the organism, separable from the protoplasm, whose mode of action we can investigate at our leisure. It is a catabolic function of the organism, and by a complete knowledge of its mode of action we should learn how the process of combustion in the animal body proceeds at so low a temperature. The action of enzymes is generally recognised as consisting in a splitting up of complex molecules into simpler, accompanied by hydration. The same result is attained by the chemical action of acids and alkalies or even by a much higher temperature alone. Thus hydrochloric acid can convert fibrin into peptone and starch into sugar. But this occurs only at 100° C., whereas with enzymes the change takes place at a very much lower temperature. The power possessed by a comparatively small quantity of an enzyme of decomposing relatively very large quantities of a special medium is spoken of as due to *catalytic* action—an action bordering on the chemical and the physical. The dependence of its optimum action on a definite temperature suggests that it may consist in the transmission of a certain molecular motion, which enables the molecule to be decomposed at a much lower temperature than if the force were less accurately adjusted to the work to be performed,—just as certain chemical substances explode most readily when sound waves of a certain length and rapidity impinge on them, or as certain rays of light decompose or cause to combine certain chemical bodies. The energy which is present in the shape of temperature, instead of acting

as it usually does for the most part in merely increasing the intermolecular spaces by increasing the mean free path of the molecule by means of the enzyme, is presented in such a form that it is taken up by the molecule or rather by a part of the molecule until its integrity is wrecked and *dissociation* occurs. Now we have already seen that the enzyme is merely a property separable from the protoplasm, but not differing otherwise from other functions. Each enzyme has its optimum temperature, and may not the different *catalysing* powers of the protoplasm of an organism have different optimum temperatures? A series of facts which have been accumulating for some time must, I think, be explained in this way. We find that the pigment bacilli have each an optimum temperature at which they produce most readily their pigment, although this does not necessarily coincide with that at which they grow most rapidly. Thus *Prodigiosus* grown at the usual temperature of the room, is of a striking red colour, but, when cultivated at the temperature of the animal body, it is absolutely colourless.* I had recently occasion to

* This need not be entirely attributed to the direct influence of the temperature on the catalysing processes; the protoplasm, as we have already seen, exhibits a selective action as regards the different substances offered it as food, and it may be that when one process is partially interfered with, a similar power may come into play, and this would exaggerate the direct effect of the temperature. Correlated with these perhaps only quantitative changes which may be catabolic or anabolic in character, the metabolism as a whole may become more or less altered, so that the centre of gravity of the organism becomes as it were shifted. It is to this, probably, that we must refer the fact noted by Schottelius that, when *Prodigiosus* is grown for a certain time at the higher temperature, it loses the faculty of producing its pigment even when grown at a lower temperature. In a more recent communication he states that, when grown sufficiently long at the lower temperature, the property returned, so that we have here an example of that form of "reversion" which Romanes has recently so ably discussed. Dallinger has found that Infusoria can be gradually accustomed to withstand very high temperatures, and this adaptation may be associated with a similar change in the metabolism. The "tolerance" which Kossiakoff (*Annales de l'Institut Pasteur*, No. 10, 1887) has shown that microbes can acquire towards antiseptic agents when previously cultivated in more dilute solutions, is to be referred chiefly to the organisms becoming gradually accustomed to exist without those "*associations*" and "*dissociations*" which the chemical substance tends to inhibit, and to a corresponding development of others to take their place. The permanence of this new habit of the organism, when grown again under the old conditions, will depend on the more or less stable nature of the new combination or complex functions which has been evolved. This modification of the organism as a whole, which may result from a change in one direction, and is dependent on

investigate the growth of a series of organisms on media coloured with litmus, and found, to my great perplexity, that a certain number reddened it, but only at a definite temperature which varied with the organism and the medium under consideration. Relatively larger quantities of acid than ammonia (?) appeared to be formed only within a certain range of temperature. Warrington * found, with quite a series of organisms, that when cultivated in milk, at higher temperatures they produced relatively more ammonia, at lower relatively more peptone. It has been recently communicated that certain yeasts produce at lower temperatures relatively more alcohol, at higher temperatures relatively more glycerine. The one *dissociation* appears to be favoured at a higher, the other at a lower temperature. I conclude that the temperature acts more or less directly on those *catalysing* processes inherent in the protoplasm itself, in a way similar to that in which it acts on the enzymes which are separable from the protoplasm. It has already been noted, that in the organisms of the cholera group the rennet-like ferment appeared able to act in acid reaction in which the pepsin-like ferment was inhibited. We should not then be surprised to find that a chemical substance is able to inhibit one *catalysing* process in the protoplasm without materially affecting the others. There are already many facts in Bacteriology which speak for this, but a systematic examination of the influence which such bodies exert on the functions of microbes is much to be desired, as throwing light upon the way in which a drug affects the reactions of the living protoplasm, and as furnishing us with a basis on which a cellular therapeutics may perhaps in the future be founded. Interesting as this subject is, it cannot now be further entered upon in a paper whose proper theme is the enzyme function in its relation to the general physiology of the cell.

In conclusion, I wish to acknowledge my great indebtedness to Professor Ferdinand Hueppe, not merely for invaluable assistance in the experimental work, but also for the general biological outlook, I have here adopted. My thanks are also due to Dr Woodhead, for

what we may term the solidarity of the organism, has never yet received the attention which it deserves as a factor which may determine the degree of fixity of a new adaptation.

* *Journal of the Chemical Society*, 1888.

much kind assistance in the work carried out in the Laboratory of the Royal College of Physicians.

A New Synthesis of Dibasic Carbon Acids. By Prof. Crum Brown.

(Read February 17, 1890.)

(*Abstract.*)

The electrolysis of potassium salts of the form $\mathrm{K{-}O{-}\overset{O}{\overset{\|}{C}}{-}R}$ in strong aqueous solution has been shown by Kolbe* to lead to the formation at the anode of R_2. $\mathrm{K{-}O{-}\overset{O}{\overset{\|}{C}}{-}R}$ decomposing into the ions K and $\mathrm{{-}O{-}\overset{O}{\overset{\|}{C}}{-}R}$; the former giving at the cathode caustic potash and hydrogen, and the latter giving at the anode carbonic acid gas and R_2. It occurred to me that if in dibasic acids, containing two carboxyls, one carboxyl could be temporarily shut off from taking part in the electrolysis, an interesting synthesis might be effected. Guthrie's observation† that $\mathrm{K{-}O{-}SO_2{-}O{-}Et}$, when subjected to electrolysis with an anode of amalgamated zinc, gives caustic potash and hydrogen at the cathode, and zinc ethyl sulphate at the anode, gave a hint how such temporary eclipse of one carboxyl might be effected. I therefore determined to try the electrolysis of such a salt as $\mathrm{K{-}O{-}\overset{O}{\overset{\|}{C}}{-}R''{-}\overset{O}{\overset{\|}{C}}{-}O{-}Et}$, in the hope that it would give at the cathode caustic potash and hydrogen, and at the anode carbonic acid and $\mathrm{(R''{-}\overset{O}{\overset{\|}{C}}{-}O{-}Et)_2}$, that is $\mathrm{Et{-}O{-}\overset{O}{\overset{\|}{C}}{-}R''{-}R''{-}\overset{O}{\overset{\|}{C}}{-}O{-}Et}$.

* *Annalen*, lxix. 257.

† *Chem. Soc. Quart. Jour.*, ix. 131.

The following paper shows that this expectation has been fulfilled.

Dr Walker and I are at present engaged in applying this method to other dibasic carbon acids, saturated and unsaturated, and hope shortly to communicate further results.

The Electrolysis of Potassium-Ethyl Malonate and of Potassium-Ethyl Succinate. By Prof. Crum Brown and Dr James Walker.

(Read February 17, 1890.)

(*Abstract.*)

Synthesis of Succinic Acid.—Potassium-ethyl malonate is easily prepared according to the directions of Freund (*Berichte der deut. chem. Gesellschaft*, xvii. 780).

The conditions most favourable for the electrolysis were found to be as follows:—A large platinum crucible formed the cathode, while a spiral of stout platinum wire was used as the anode. The source of electricity was a battery of accumulators, and the current was so regulated that while passing through the solution it had an electromotive force of 12 volts and a strength of not more than 5 ampères. Under these circumstances the heat developed in the solution could be easily conducted away by a stream of cold water flowing round the platinum crucible. Heating is to be avoided, on account of possible saponification of the potassium ethyl salt by the action of the caustic potash formed at the cathode. With the above arrangement, however, the hydrogen developed at the cathode serves to stir up the liquid and bring the potash into contact with the carbonic acid liberated at the anode, and thereby convert it into carbonate, which, far from being prejudicial to the action, seems on the whole advantageous. One point to be attended to is that the solution should not be too concentrated. When this is the case the liquid has a high resistance, and the electrolysis proceeds slowly; the product, too, is not so satisfactory as when the solution is so far diluted that a brisk evolution of gas takes place at both poles without any excessive frothing being occasioned by the viscosity of the

liquid. After the current has passed for some time, a deposit begins to be formed in the solution. This is mainly potassium carbonate, with possibly some bicarbonate. When this has increased somewhat in quantity the current is broken, and the contents of the crucible transferred to a separating-funnel, where they are shaken up with ether. A further quantity of carbonate is thereby precipitated. The aqueous layer is then allowed to run out, nearly all the solid carbonate remaining behind, and the ethereal layer is poured off. The former is again submitted to electrolysis, and the extraction with ether repeated. The ethereal liquids are united, dried with calcium chloride, and the ether distilled off on a water-bath.

In our first experiment, made with 15 grams of the double malonate, there remained in the distilling-flask over 3 grams of a nearly colourless liquid of ethereal odour. This liquid was subjected to fractionation; it passed over almost entirely at 213° (uncorr.): the boiling-point of succinic ether is 216°. A portion of the distillate was analysed, with the following results:—

·2619 gr. substance gave ·5287 gr. CO_2
and ·1930 gr. H_2O

	Found.	Calculated for Succinic Ether.
C	55·06%	55·17%
H	8·19	8·05

There was thus little doubt that the substance so obtained was succinic ether. In confirmation, however, a portion of the ether was saponified, and the white silver salt precipitated. This salt was then carefully ignited, and the residue of silver weighed.

·1878 gr. substance gave ·1215 gr. silver.

	Found.	Calculated.
Ag.	64·7	65·0

Synthesis of Adipic Acid.—Heintz (*Poggendorff's Annalen*, 108, 82 [1859]) obtained potassium-ethyl succinate by treating succinic anhydride with absolute alcohol, neutralising with potassium carbonate, and precipitating the double salt from its alcoholic solution with ether.

The conditions to be observed in the electrolysis are quite the same as in the case of potassium ethyl malonate, and the mode of

extraction is identical. The residue left on distilling off the ether on the water-bath is a liquid of pale straw-colour, with a pleasing but faint odour of melons. It boils with decomposition at about 240°. An analysis of the substance thus obtained, after drying in an exhausted desiccator, gave the following numbers:—

·1735 gr. substance gave ·3770 gr. CO_2
and ·1423 gr. H_2O

	Found.	Calculated for Adipic Ether.
C	59·26	59·41
H	9·11	8·91

The product thus appears to be practically pure adipic ether. From 70 grams potassium ethyl succinate 15 grams of adipic ether were obtained.

A portion of the ether was saponified with alcoholic potash, and from part of the potassium salt thus produced the silver salt was precipitated, while another part was converted into the acid, which was purified by shaking its ethereal solution repeatedly with small quantities of water. The acid melted at 147°: the melting-point of adipic acid is 148°. The white silver salt was analysed for silver with the following results:—

·2388 gr. silver salt gave ·1428 gr. silver.

	Found.	Calculated for Silver Adipate.
Ag.	59·8	60·0

The Action of Sodium Carbonate and Bromine on Solutions of Cobalt and Nickel Salts. By Dr John Gibson.

(Read February 17, 1890.)

(Abstract.)

In 1862 Field gave a brief account of a peculiar green solution, prepared by adding nitrate of cobalt to a solution of bicarbonate of soda containing a small quantity of the hypochlorite of that alkali. The author observed the formation of a similar green solution when making qualitative separations of chromium from other members of

the iron group by means of sodium carbonate and bromine, and was thereby led to investigate the action of these two reagents on solutions of nickel and cobalt salts.

Green Cobalt Solution.—If sodium carbonate is added in large excess to a solution of a cobaltous salt, on shaking the mixture with a sufficient quantity of bromine, the whole of the precipitated cobaltous carbonate dissolves, giving rise to a beautiful dark green solution. This solution is stable when preserved in closed vessels at ordinary temperatures. It decomposes on boiling with precipitation of cobaltic oxide. Caustic alkali produces rapid decomposition, cobaltic oxide being precipitated. The green colour is destroyed on acidifying with hydrochloric or sulphuric acid, but reappears on neutralising the acidified solutions with sodium carbonate. Acetic acid does not decolorise the green solution.

Red Cobalt Solution.—If the green solution, prepared as above, is acidified with sulphurous acid, and the decolorised solution then rendered alkaline with sodium carbonate, a fine red-coloured solution is produced. On shaking this in presence of air, it absorbs oxygen and becomes green. On standing, the colour goes back to red, but, on again shaking with air, becomes green. These changes of colour may be produced a number of times. After some time, or on adding alcohol, the red solution ceases to become green on shaking with air.

The reaction of solutions of nickel salts with sodium carbonate and bromine are very complex. They vary in a remarkable manner, according to the temperature and concentration of the solutions employed and the relative proportion and order in which the reagents are added.

If excess of sodium carbonate is added to a solution of nickel, the resulting mixture behaves differently on addition of bromine, according to the proportion of bromine added. If a large excess of bromine is added, part of the nickel goes into solution, part remains undissolved as pale green carbonate. On the other hand, if a smaller proportion of bromine is added, so as to leave excess of normal sodium carbonate, the nickel is rapidly and completely converted into peroxide.

If bromine is added to a strong solution of sodium carbonate, and a small quantity of a dilute solution of a nickel salt is poured into

the resulting mixture, the whole of the nickel goes into solution. Such solutions are, however, very unstable, and rapidly darken, owing to the formation of peroxide of nickel. Much more stable solutions of nickel can be obtained by using supersaturated solutions of sodium carbonate, prepared by subjecting decahydrated sodium carbonate to aqueous fusion.

The author points out in this connection that bromine does not liberate carbonic acid from such a supersaturated solution of sodium carbonate even when it is added in excess. Carbonic acid is, however, given off freely on subsequent dilution with water.

The author is at present investigating the action of bromine on sodium carbonate under varying conditions—(*a*) upon each other, (*b*) upon mixed solutions of cobalt and nickel salts.

His experiments, so far as they have gone, point to the possibility of effecting the separation of nickel from cobalt in a rapid and easy manner.

On certain Substances found in the Urine, which reduce the Oxide of Copper upon Boiling in the presence of an Alkali.* By Herbert H. Ashdown, M.D.

(From the Physiological Laboratory of University College, London, and the Laboratory of the Royal College of Physicians, Edinburgh.)

(Read December 16, 1889.)

(*Abstract.*)

As the immediate result of the ingestion of certain chemical compounds—chloral, camphor, benzol, phenol, &c.—substances make their appearance in the urine which have long been known to possess the power of reducing the oxides of some of the metals when in solution in the presence of an alkali, if that mixture be raised to the boiling point, but considerable confusion has resulted from the difficulty of finding a short process whereby many of these substances may be readily recognised as not belonging to that comprehensive group of conditions generally styled as glycosuria.

The importance of this group has been greatly increased of late

* A grant was given by the British Medical Association towards the expenses of this research.

years, however, by the attempts made to define and differentiate the individual members of which it consists, and to ascribe to each their relative significance, and these efforts have been crowned in some measure by a considerable degree of success.

Schmiedeberg and Mayer were the first to isolate and analyse the substance thus produced, and found it to be *glycuronic acid*, and that its chemical constitution was represented by the formula $C_6H_{10}O_7$.

This substance appears in the urine in combination with urea, from which it may be obtained by precipitation, by means of barium hydrate, and extraction with alcohol, and decomposition of the compound thus obtained by sulphuric acid.

It holds the oxide of copper in solution in the presence of an alkali and reduces it, throwing down the suboxide, upon boiling ether in Trommer's or Fehling's tests; and a similar reaction occurs with the oxides of bismuth, mercury, and silver.

For the definite recognition, however, of *glycuronic acid*, the only reliable means possessed at present is to thus obtain it pure, but, as already remarked, the process is long and tedious.

The polariscope, unless used with pure solutions of this acid, or with solutions of known combination, is very apt to mislead, since some of its combinations rotate the ray of light to the left, and others to the right; and if glucose be also present very erroneous conclusions may be arrived at.

When pure, it rotates the ray of polarised light to the right,—35°,—or to half the extent of the deflection produced by glucose.

There is no doubt, however, that with comparatively small quantities of urine the differentiation of this substance from the glucose, which possesses similar chemical reactions, may be readily arrived at by applying the test of fermentation by yeast—a test of great delicacy if conducted properly and with due care over mercury, and one capable of demonstrating the presence of smaller quantities of glucose than either Fehling's solution or Trommer's test can detect.

Experiments.

I have now investigated a large number of specimens of urines obtained under different conditions, in order to trace as far as possible the presence of *glycuronic acid*, and to differentiate it from glucose. The fermentation test was always employed, and, whenever

the quantities of urine obtained were large enough, the chemical process was undertaken in its entirety for obtaining the acid pure. I now chronicle my results:—

Morphia.—The urine secreted after the exhibition of this drug, either by the mouth or subcutaneously, contained a substance, which, from its power to reduce Fehling's solution, has been generally regarded as a transient condition of glycosuria, but after a close investigation I find it to be due, not to the presence of glucose, but of *glycuronic acid.*

Chloroform.—I have also satisfied myself that the reducing power of the urine, after the administration of this anæsthetic, is dependent entirely upon this acid, and is not due to glucose. In this manner I have been able to confirm independently the observations of Mayer upon morphia and chloroform.

With chloroform I have, however, found some exceptional instances, in which no reducing substance appeared in the urine, but these exceptions over a large number of observations were undoubtedly rare.

Curara.—The glycosuria, so called, of curara poisoning has long been known, having been first observed by Claude Bernard, but I have not succeeded, after an extended series of observations, in obtaining any fermentation by yeast of these urines, which appear otherwise to be so markedly glycosuric.

In these instances, however, it was most difficult to obtain large enough quantities of urine to complete the whole chemical process, since the curara itself interferes so decidedly with the secretion of the kidney; and consequently I have not been able to separate out the acid in this group of observations.

Ether.—As the result of the administration of ether I have never found the appearance of this acid or of any reducing substance in the urine, and it is therefore a useful anæsthetic for observations upon the urinary system.

Analysis of Urine secreted by the Right and Left Kidney.

Renal Nerves Intact.—With the view of further investigating the significance of this glycuronic acid, I placed cannulæ in the ureters, in order to collect for analysis the urine so secreted by the two kidneys separately. I found that under ordinary conditions the

chemical constituents of the different secretions differed only to such an extent as could be readily accounted for by the difference in the size of the two organs.

I found, as the result of these experiments, that if the reducing substance was present in the urine of one side, it was invariably found also in that of the opposite side.

Section of the Renal Nerves.—Again, in another series of experiments, I collected and analysed the right and left urinary secretions separately after division of the renal nerves upon the left side. These observations then differed from those of the preceding group, only by the fact that one organ had been completely cut off from all nervous influences from any source without the organ itself.

The results which were thus gained are most interesting, and an illustrative series of these experiments are tabulated below.

TABLE I.—*To show the Appearance of the Reducing Substances in the Right and Left Urinary Secretions.*

No. of Observation.	Duration of Observation.	Anæsthetic.	Urine.		Remarks.
			Right.	Left.	
I. 1.	60′	Ether.	o	o	The renal nerves
2.	75′	,,	o	o	were divided
II. 1.	160′	Ether and Chloral.	+	+	upon the left
III. 1.	360′	Ether and Morphia.	+	+	side in each
IV. 1.	120′	Ether and Morphia.	+	+	observation.
2.	120′	,, ,,	+	+	+ indicates the
3.	120′	,, ,,	+	+	appearance of
V. 1.	120′	Chloroform.	+	+	reducing sub-
VI. 1.	60′	Ether and Chloroform.	lost	o	stance.
2.	90′	,, ,,	o	+	
3.	120′	,, ,,	o	o	
VII. 1.	30′	Ether and Chloroform.	o	+	Trace.
2.	120′	,, ,,	o	+	Markedly.
3.	180′	,, ,,	o	+	Slight.

Ether.—Where this was the only anæsthetic employed, I never succeeded in demonstrating the presence of this reducing substance in the urine (Expt. I.). But when used in conjunction with chloral hydrate (Expt. II.) or morphia (Expt. III., IV.), it was present in such abundance as to be very readily recognised.

This result was produced upon both sides, and although I am not able to state that I could recognise any very marked increase in the

amount excreted upon the left side, no quantitative analysis having been made, it certainly was present in larger quantity than upon the right. These results may be explained as corresponding to the results gained previously and already noted.

Chloroform.—As was to be expected when this anæsthetic was exhibited (Expt. V.), the reducing substance manifested itself in abundance upon both the injured and also the uninjured sides.

Ether and Chloroform.—In the instance in which these two anæsthetics were combined, a very interesting result was gained (Expt. VI. and VII.).

If the chloroform was given to any pronounced extent both sides showed the reducing substance, but in those instances where the chloroform was only given at the earliest stages, and its administration stopped before the commencement of the collection of the urine for analysis, a different condition appeared to be induced.

Under these circumstances no reducing substance was present in the urine secreted by the normal and uninjured kidney, but in the urine which flowed from the other kidney, the nerves of which had been previously divided, this reducing substance was readily demonstrated.

In the case of Experiment VI. it was found on the left side only, but there only in small quantity, and limited apparently to only a short period of time (VI., L. 2), for none could be detected, not even a trace, in the first or last stage of the observation. No traces could at any time be obtained on the right side at any stage.

In Experiment VII., however, a further point is illustrated. This substance appeared distinctly throughout the whole period of observation in the left secretion, *i.e.*, the side upon which section had been performed—but much more markedly during the second period (VII. L. 2), during which it seemed to reach its maximum and to be present in quantity, and then again to fall off. There was no trace detected on the right side.

In these instances, therefore, it was found that this reducing substance appeared upon the left side only,—upon the side where complete division of the renal nerves had been effected,—and also that the greatest output, in the former instance (VI.) the only output, was found to correspond with the period of the greatest activity of the renal epithelium, if the eliminative activity may be judged of

by the respective quantities of the normal urinary constituents eliminated.

I have shown elsewhere that the renal secretion, after section of the renal nerves, is a true paralytic secretion, and I find that the greatest abundance, never very large though distinctly marked, of this substance occurs at the maximal display of energy which is reached just prior to the outset of the stage of exhaustion.

After a very careful and exhaustive chemical analysis of these different specimens, I have convinced myself that this substance which is present is not glucose, and that this condition is not therefore a form of glycosuria properly so-called, and I have little hesitation in affirming that it is due to the presence of glycuronic acid.

It is, however, no easy task to seek out the cause or significance of these results. The animals were fed with a liberal meat diet on the evening prior to the observation, and as the urine showed no tendency to reduce solutions of copper before the experiment, it is necessary to conclude that this reducing substance is occasioned by the disturbance of the economy resulting from the experiment itself.

Again, since the administration of chloroform is now recognised as able to establish such a condition in healthy animals by mere inhalation, and as I have never succeeded in producing these conditions without having employed this anæsthetic, I am disposed to attribute the appearance of this reducing substance to the effect of the chloroform inhaled, although it does not at present seem possible to appreciate in what particular manner or by what altered process of metabolism it is brought about.

The explanation, further, of these appearances in these results upon one side only, does not seem to be very simple, but two theories may be offered as capable of accounting for these results—firstly, this reducing substance may be circulating in the blood as the result of the conditions under which the animal experimented upon has been placed, and that it may be eliminated by the one kidney alone, since the action of the cells of that organ are completely cut off from the central nerve influences, which hold the other organ in check; or, secondly, this substance may be formed in the cells of the one organ alone, owing to the fact that there is a greater activity produced in them by section of the nerves,

and this is accompanied by a much greater supply of blood to the organ, which is synonymous with an increased supply of material from which the substance is formed.

It has already clearly been shown that a condition may be established in the which both kidneys will produce a similar result, and it is therefore difficult to imagine that this material is already in the blood, and is being eliminated upon one side only; but it is not unreasonable to consider that, if only a small supply of material be forthcoming, the organ which has the greater supply sent to it, accompanied by an increased activity in the cells themselves, will produce the results described.

I lean to the latter view, since the quantity in which this reducing substance appears in the left secretion is sufficiently abundant to lead one to expect its appearance in the right secretion, if the substance were circulating in that form in the blood itself.

I therefore advance the view based upon these observations, that there is a distinct chemical process presided over by the renal epithelium which has as its result the formation of *glycuronic acid*, but the precise nature of that process and upon the presence of what chemical compounds it is dependent I am at present unable to appreciate.

If this acid may be regarded, as is probably the case, as a derivative of an oxidation process of the sugars within the animal economy, these results may prove of considerable importance as offering an explanation of how the sugars are oxidised within the system.

These observations were made in connection with the lower animals, and I was thus encouraged to further pursue my observations in regard to man.

After a prolonged search over several hundred cases of man, both in disease and health, I have succeeded in finding one instance —this is the first and only recorded instance, I believe—in which this reducing substance has been shown to be excreted in large quantity.

This individual is a man about twenty-four years of age, who apparently enjoys perfect health and a complete sense of well-being, and does not suffer from any of those symptoms which but too readily indicate the presence of glycosuria in youth.

He is, however, passing daily very large quantities of *glycuronic acid* in a urine which is not increased in quantity or density.

It must not, however, be concluded from these results that the presence of this substance is so extremely rare, for my observations were made upon individuals who were believed to be free from diabetes mellitus, and since it causes no apparently inconvenient symptoms its presence may be more frequent than one might be led to suppose.

Further, my having shunned all cases of acknowledged diabetes, I may have unconsciously rejected instances due to the presence of *glycuronic acid* in the urine which have been supposed to be glucose. It is not unlikely also that some cases of true glycosuria may have been present also in the urine *glycuronic acid*, in which case they will very effectually mask each other.

The difficulties to be met with and overcome in order to demonstrate the presence of both substances in the same urine are extremely great, and I may here state that the usual solutions of lead or baryta, which are recommended to be added to urine for the purposes of clarification before using the polariscope to estimate the quantity of sugar present, are not sufficient to eliminate all chances of fallacy in the event of *glycuronic acid* being present as well as glucose.

The Volcanic Eruption at Bandaisan. By C. Michie Smith. (With a Plate.)

(Read January 20, 1890.)

The principal phenomena connected with the great volcanic eruption at Bandaisan, in Japan, have been described by Professor S. Sekiya and Mr Y. Kikuchi in their official report,* but certain features, especially with reference to the effects of erosion on the ejected materials, seem worthy of more detailed description.

To make the account intelligible, it will be necessary to describe briefly the chief features of the eruption. The name Bandaisan is given to a group of peaks lying in lat. 37° 6′ N. and long. 140° 6′

* *Trans. Seismological Society of Japan*, 1889, and *Journal of the College of Science*, Imperial University, Japan, vol. iii. part ii.

E. Before the eruption there were four peaks—Obandai, Kobandai, Kushiga-mine, and Akabani-yama. They are of volcanic origin and, according to tradition, formed at one time a single mass which was split into four by a great eruption early in the ninth century, at the time when Lake Inawashiro was formed. Bandaisan rises to a height of 6037 feet above sea-level, and Kobandai is believed to have reached almost exactly the same altitude. For about ten centuries the mountain had shown no signs of activity, except by the existence on it of a number of hot springs and solfataras. It was, however, included by Professor Milne in his list of the active volcanoes of Japan.

The recent eruption took place on the 15th July 1888. There was no premonitory warning of the catastrophe except some rumblings heard at 7 A.M. of that day, which were followed by an earthquake of no great intensity. At 7.45 a great explosion took place, by which an immense cloud of steam and débris was shot up to a great height above the top of Obandai, and this explosion was quickly followed by fifteen or twenty minor explosions. The result of these explosions was that practically the whole mass of Kobandai was shattered, and the materials forming it were spread over an area of 27·31 square miles. As calculated by Professor Sekiya, the volume of material moved by the explosion was 1587×10^6 cubic yards, or say a cone with a diameter of 1000 yards at the base and a height of 760 feet.

The shattered fragments of the mountain travelled in two directions. The main stream went northwards, spreading out as it advanced, overwhelming a number of villages and hamlets, and entirely damming up the Nagasegawa, the chief feeder of Lake Inawashiro. The other stream took a south-south-easterly direction over the flank of Kushiga-mine and down the valley of the Biwasawa towards the village of Miné, part of which was swept away. This may be called the Miné stream. The eruption was due simply to an explosion of steam, no lava or pumice being ejected, and the so-called "mud" which spread over the country was simply the matter which had formed the mountain, more or less moistened by condensed steam and by water which had existed in the mountain, but most of it was certainly not wet enough to deserve the name of mud. The scattered materials may be described as being for the

most part earthy, mixed with stones of all sizes; but at certain places, mostly near the crater, there were great piles of large blocks of stone almost free from earth. This was specially the case at the N.E. corner of the crater, where the crater-wall showed a magnificent section of beds of lava intercalated with layers of scoriaceous material, lying unconformably on each other.

The manner in which the fragments of Kobandai travelled outwards resembled closely the rush of water from a reservoir on a hill when the embankment has burst. The stream followed, as a whole, the line of least resistance, but after descending a considerable way, and having acquired a very high speed, part of it flowed up hill again, and where, in descending the valley, any mountain spur met it at a sharp angle, it rushed up the hill side in some cases to a height of as much as 120 or 150 feet above the general level. In one place the torrent passed over a col about 200 feet high. The average velocity is given by Sekiya as 48 miles an hour. In one respect this earth-torrent behaved differently from a torrent of water, viz., in the way in which, owing to internal friction, it came suddenly to rest as if it had all at once been solidified. This is very marked in the case of the Miné stream, which ended in a bank with a nearly vertical face just above the village.

In addition to the damage done by the earth-torrents, the forests all round, except where sheltered by hills, were almost entirely destroyed by the eruption. Most of the trees were uprooted or broken off, and the few that remained had not only every leaf and twig removed, but even the whole of the bark was stripped off from them on the side facing Kobandai. This destruction of the forests was ascribed to a gust of wind caused by the eruption. That the sudden liberation of a great volume of steam must have given rise to a strong air blast is undoubted, but that all the observed effects could have been produced by such a blast seems highly improbable. This was one of the points which I took up when I visited Bandaisan, in May last, in company with Dr C. G. Knott. We examined a number of the stumps left standing, and found that in every case the trunk on the side facing Kobandai was pitted with holes of all sizes, evidently caused by the impact of stones. In some cases stones of considerable size were found imbedded in the wood. This indicated pretty clearly that the damage to the trees had been

caused largely by what may be described as a nearly horizontal hail-storm of small stones. Some idea of the density of this storm was obtained by counting the number of marks on a measured area. This was found to be from 250 to 300 per square foot, and it must be remembered that most of these were probably made after the bark had been removed by the first part of the storm. At a distance of 3 miles from the crater the bark had been stripped from only very young trees, but the marks of blows could easily be seen on the bark of all the trees.

In following the earth-torrent from Miné up the bed of the Biwasawa, I was immediately struck by the extraordinary effects produced by erosion in the short space of ten months which had elapsed since the eruption. The Biwasawa is only a small stream, and yet in these ten months it had carved out a valley which at one place was, by actual measurement, 80 feet deep and 80 feet wide at the top, and at other places, where measurement was impracticable, was by estimation little short of 150 feet deep. It must be remembered, too, that this one cutting does not represent nearly the whole of the work done by the stream, which had not flowed along the same channel all the time. Had the stream flowed quietly along its channel for the whole of the ten months, it is probable that it could not have made nearly so deep a cutting, but it was evident that much of the work had been done by a succession of floods. In one place, for instance, a lake had been formed which, after reaching a large size, had burst through the embankment which held it back, and the water descended the valley with an impetuous rush, carrying even stones of a considerable size along with it. In other cases landslips had blocked the channel at various points for some time, and during the winter snow-slides had also aided in forming temporary dams. Even at the end of May the stream was spanned at several places by snow bridges. The material through which this cutting was made was, of course, very soft, and in many places it had been reduced almost to the consistency of mud, for all the small tributary streams had been blocked by the earth-torrent, and the water was taken up as by a sponge by the mixture of loose earth and stones. The photograph reproduced in the Plate shows some of the main features of the erosion but, unfortunately, the heavy mists which hung about the hills, lifting

only for a few minutes at long intervals, made it impossible to get really good views. The great V-shaped cutting is, however, clearly seen, and it should be noted that, judging from photographs taken soon after the eruption, and from the descriptions given by visitors, this cutting is entirely due to erosion.

The minor details of the water action were no less interesting. The loose débris was, as has been mentioned, mixed with large blocks of stone. One of these, distant some 2½ miles from the crater, measured about 18 feet by 13 feet by 13 feet, and still larger blocks were met with higher up. Now when the water had cut down to such a block it behaved in one of several ways. In some places, where the eroded channel was wider than the block, passages for the water were cut round one or both ends, and the block itself gradually sank down as the material on which it was supported was cut away. In some cases, where the block was long enough to bridge the channel, a tunnel had been made under it through which all the water passed. In other cases waterfalls of considerable height had been formed by such blocks. In these cases the large block was protected on its upper side by a number of smaller stones, which prevented erosion from taking place behind it. The consequence of this was that the bed of the stream for some distance back became nearly horizontal, and the carrying power of the water was greatly decreased, thus tending to make the falls permanent. It is not to be supposed that they will be really permanent, as the blocks will gradually be undermined by the water falling over them, but their existence at present illustrates the way in which differences in slope in the bed of a torrent may have been brought about by obstacles which have long since been removed. As seen from above, there were three main lines of erosion, but that along the old bed of the Biwasawa was by far the most prominent. In addition to these principal channels there were numberless tributary channels, reproducing in miniature all the details of mountain sculpture.

In a few years the materials forming the earth-stream will consolidate into what will probably be classed as a volcanic breccia, and erosion will go on much less rapidly. The consolidation has indeed already begun, and it was interesting to notice how a sort of laterite was forming out of the more ferruginous materials, which

were in parts plentiful. Vegetation, too, will before long help to protect the surface, but at the time of my visit the only traces of this that could be found were a small fern and a small plant of lichen on the warm ground close to a nearly extinct fumarole in the crater itself.

In and near the crater weathering showed itself in other forms. Immediately after the eruption the walls of the crater were nearly vertical, rising in some parts to a height of over 1600 feet above the crater floor. In the more rocky parts this has not been greatly changed, but elsewhere, through the action of rain and frost, constant landslips have been taking place, and the precipices are now no longer vertical, but are for the most part covered with a talus lying at the angle of repose. Again, the rugged "conical mounds," of which hundreds were spread over the crater and the main earth-stream, forming a most conspicuous feature in the early photographs, are now smoothed and rounded to an extent almost inconceivable in so short a time. Another striking feature is the rapidity with which many of the rocks lying about the crater are crumbling down. These have been subjected to the action of steam and acid vapours in the heart of the mountain, by which almost all the constituents except silica have been removed.* When exposed to the air these blocks quickly fall to pieces, spherical flakes which soon fall into sand peeling off them with great ease.

It is worth considering to what extent the gradual decomposition of the rock brought about the final catastrophe. The mountain was, as it were, bound together by a number of sheets of lava, which made it strong enough to resist the steam pressure beneath. During the past ten centuries these lava beds have been gradually decomposed along certain lines, and at length a time came when they were no longer able to resist the steam pressure, and the mountain was blown to pieces. May not this, rather than the sudden development of a large quantity of steam, as usually supposed, be the true history of the eruption?

The eruption of Bandaisan is certainly not the first of the kind that has occurred in Japan. Professor Sekiya has pointed out some

* Analyses made by Mr Shimizu give the proportion of SiO_2 in this rock as 91·66 per cent., while in the natural rock it was only about 59·6 per cent.

EROSION ON THE MINE EARTH-STREAM.

others which appear to resemble it closely, and in travelling through Japan I came upon another very striking example of this kind of action. This was near Ikao—famous for its hot springs—where there is an old crater of large size, one wall of which has been completely blown away and scattered over a considerable tract of country. Standing on the top of the remaining crater-wall, one can still trace the path of the torrent of débris, and the section made by a stream which passes through it shows no signs of lava, but only materials similar to those found at Bandaisan. The surface is now covered with vegetation, but there are few trees on it, and such as there are are low and stunted, while it is surrounded by healthy-looking woods.

On Evolution and Man's Place in Nature.
By Professor Calderwood.

(Read January 27, 1890.)

My aim in this paper will be to present as concisely as possible the problems involved in Man's Place in Nature, and to consider briefly how far an evolution theory contributes towards the solution of these problems.

Needful preliminaries can be disposed of briefly. We are agreed that evolution is "a change from an indefinite, incoherent homogeneity to a definite, coherent heterogeneity, through continuous differentiations and integrations." We are agreed that this process is to be taken as applicable in the history of matter and motion, and afterwards in the history of organised existence, raising the whole problem of biology. We do not require here to linger over the transition from the one to the other, as all requirements are met by accepting life and its laws as facts, and acquiescing in Darwin's hypothesis of one or more primordial germs. Next, we take Darwin's laws as applicable in the history of organism. These are briefly—(1) struggle for existence, with survival of the fittest; (2) adaptation to environment; (3) hereditary transmission of acquired adaptations. We do not need to raise the debate between Darwin's view and Weismann's as to hereditary transmission, with its two-fold bearing, as it concerns mode of transmission and the time

needful for securing results. Granting, as a hypothesis, biological evolution under the conditions stated, with reservation of all the open problems concerned with the history of evolution, the way is clear for considering how this theory of evolution stands related to man's place in nature.

While discussing the question from the standpoint of evolution, we must accept Herbert Spencer's view as correct, which takes evolution of *existence as a whole* as indicating the true range of the problem raised. "Evolution becomes not one in principle only, but one in fact," implying "one evolution going on everywhere after the same manner." Further, we take it as beyond question that man belongs to Nature, and in respect of his whole being comes within the scope of science. An evolution theory must include man, or acknowledge that it is not a theory of existence as a whole. The position of the theory and of all scientific observers devoted to it is clear. Every piece of scientific work, whether concerned with structure of organism, with functional activity, with visible adaptations of organism, or with manifestations of animal intelligence, is an essential contribution towards solution of our problem. On the other hand, everything which tends to clear up the specialties of human life, whether concerned with the structure and functions of human organism, or with the activities of human intelligence as these are concerned with the attainment of scientific knowledge and the government of conduct, is a definite contribution towards the scientific conclusion we seek to reach. The outstanding problem is this,—What is man's place in nature?

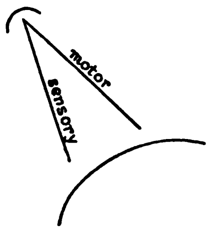
Scientific inquiry advances towards this problem along the pathway of biological research, always making account of the common characteristics of life. The common condition of organic existence, from the lowest form to the highest, is the sensori-motor nerve-system. The main question is therefore definitely shaped,—What is a sensori-motor system equal to? How much can be scientifically made out as lying within range of such a system when highly differentiated and co-ordinated in an elaborate nerve-centre, or series of nerve-centres, liable to modification in the history of its activity? According to the answer to this inquiry is the advance made towards including man within a science of nature.

The laws of a sensori-motor system, as it provides for the activity

of life, are well defined. In accordance with these every organised being is placed in vital relation with its environment; it is sensitive to contact, it is capable of receiving sensory impression, and of responding to it in action. This may be represented in simplest form thus :—

Centre of Vital Energy.

Apparatus.



Environment.

Starting with this simple provision, research is continued along the line of increasing complexity in structure, until we find Special Senses, with special terminal organs providing for definite modifications of the sense of touch, while the elaborate system of nerve-fibres is correlated in a nerve-centre of proportionate complexity.

We come within the region of difficulty and debate when we reach the higher orders of animal life, classifying these together as we may do in accordance with homologies in structure and function. Within this group we may include with man, our higher domesticated animals, the cat, dog, and horse, and besides, the monkey and ape.

We are thus brought directly upon the question of animal intelligence, with all the difficulties of observation connected with it. Here at least we have definite evidence of intelligence, for which it is needful to account. And this problem is far from easy, in view of the forms of life only a little lower. A tendency has appeared to seek abatement of this puzzle by the hypothesis that mind is present from the first movements of life. There is extreme difficulty in interpreting such a suggestion, as we recognise in representing mind-manifestation in a gnat, in a worm, or in a mollusc. Besides, the hypothesis virtually abandons the scientific conception of evolution, giving up the attempt to demonstrate that the appearance of intelligence is scientifically explained through elaboration and differentiation in the nerve-centres. If it is held that "psychic"

action is the constant attendant of vital action, and is the true explanation of it, we transform the theory of existence so as to represent it as a single life history, unfolding itself more fully as the ages roll on,—moving through matter, and next through spirit, back to the Idea, as Hegel represents. But the scientific conception makes intelligence a later type of being, evolved in the history of organic advance. In this light we read the theory of evolution.

We are therefore required to concentrate attention on the classification of the higher animals already given, so as to trace the appearance of intelligence in our world's history. As we contemplate these higher animals, first in their relation to lower orders of life, and next in their relation to man, it seems plain that we have to deal with three sets of facts,—sensori-motor activity in the simpler organic forms; a simpler or lower order of intelligence, as in the dog; and a higher intelligence, in possession of man. It would seem that no scheme of natural history can meet requirements, if it fail to make account of these three stages of life, or three distinct sets of facts.

The recognition of this sufficiently clears the path, carrying us forward to the highest form of the problem, concerning man's place in nature, without our being entangled and delayed with the question concerning animal intelligence. Our own intelligence, as the better known to us and the more easily studied, can supply the essential phases of this problem, and can be discussed without prejudice to the intervening and lower type of intelligence. The problem is clear and definite,—How can sensori-motor action evolve a rational activity? Or, taking the problem, in the first instance, on the side of intelligence alone,—How can nerve sensibility provide for evolution of intelligence? That this has been matter of actual history is fast becoming the traditional belief of thorough-going evolutionists. Scientific tests, therefore, need to be applied with exactness here, seeking for facts and their interpretation.

Given a highly-developed organism, with large adaptation to environment, and modified under long application of the law of heredity, to account for the rise of intelligence as exercised by man. The problem is certainly not an easy one, though the popular scientific faith shows no sign of misgiving. Accepting all the conditions as laid down by the evolutionist, there still seems a set of difficulties

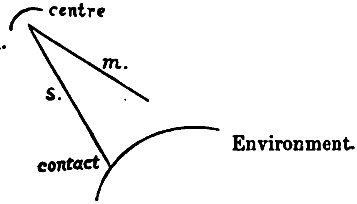
of a seriously perplexing order. These I shall attempt shortly to describe, for they do not seem to be at all lessened by the most recent investigations and discussions.

There is practical unanimity—at least ample agreement to sustain our scientific conclusion,—concerning the structure and functions of the sensory apparatus. We may experience some difficulty in representing to ourselves what *feeling* is, as it appears in the life of a snail or of a fish; but we are at least clear that there is in both forms of life sensibility to contact, and such sensibility as proves adequate to direct motion. We can similarly interpret sensibility along divergent lines, as in the action of the optic, auditory, and olfactory nerves in higher life-forms. An animal swerves as readily under the influence of light, sound, or odour, as under the whip; there seems no manifestation of intelligence in this, as there would be none in the case of a man. But intelligence is something very different from sensory impression, whether it is concerned with the size of an object, direction of a sound, or the meaning of sign. How can we explain the appearance of intelligent action?

The functions of the best developed sensory system seem quite inadequate to supply a scientific conclusion here. Even if we add all the advantages connected with an exact knowledge of intelligent experience, as in our own consciousness, our difficulties are not abated, but seem to come out in more perplexing form. Take a series of sensory impressions, even according to the possibilities of the highest organism, and it seems impossible to find, either in their nature or in their relation, anything helpful towards an explanation of the rise of intelligence. The sensory apparatus can do no more than supply impressions; successive impressions cannot even become a series without the action of intelligence in comparing the impressions. In whatever direction we turn, with the best apparatus in most efficient order, we find that intelligent comparison is a necessity —a presupposition—in order that any such experience as ours may be possible. Impression must indeed follow contact. By the same necessity, but with no provision for permanency of the first, or for comparison of the two, a new impression must result from renewed contact. The history of this is continual rise and fall of feeling,— continual flux,—but no meaning. Sensations are continually arriving and continually passing away. Even to say as much as

this implies intelligent discrimination, for "arrival" and "disappearance" are conceptions belonging to a self-conscious being. A series is orderly succession, known and contemplated as such. This knowledge organism cannot supply. In the history of three successive impressions, No. 1 is displaced to make room for No. 2, and this in turn disappears that No. 3 may appear. Impression occurs, and the occurrence ends.

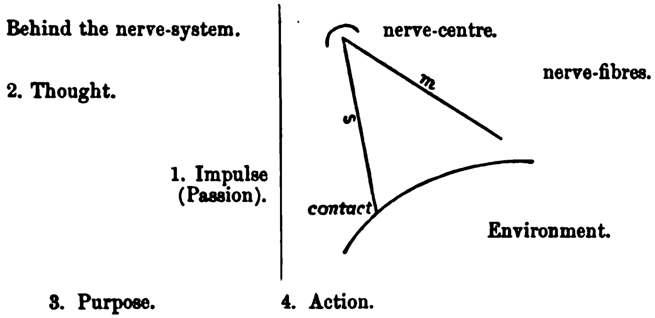
1.(Sensation). 2.(Sensation). 3.Sensation.



If now we include motor activity with sensibility, so making account of both sides of the sensori-motor system, a similar chasm seems to separate functional action from rational activity. The movement of the foot and the utterance of a word are actions widely apart. To express thought you must have thought to express; whereas, to move a muscle, no more is required than a sensori-motor system, stimulated by contact. The problem is how to account scientifically, *first*, for action provided for by nerve stimuli; and, *second*, for action provided for by intelligence as its necessary condition. We seem hardly ready even yet for grappling effectively with this problem. A moderate selection of passages from scientific works of the day would demonstrate that we have not reached a recognised technical definition of "voluntary action." Perhaps this can be accounted for in a satisfactory way by the present position of science; but as long as this want of exact definition continues, the leading problem of our age must be obscure and ill understood. What we need is, to define with scientific precision the difference between rational conduct and motor activity,—the contrast which the popular mind sees between movement and conduct, as when we speak of a man's gait as peculiar, but say of his "conduct" that it is wrong. An immense distance separates these two. Within this space lies some of the most serious perplexities the hypothesis of evolution has to encounter. What is yet to be explained is that which Aristotle signalised as "deliberate pre-

ference," a preference which is the outcome of reflection, forecasting possible consequences.

A reference to the simple diagram already used will illustrate our difficulties, when nerve-action and intellectual action are viewed as distinct.



The functions of the sensori-motor system are so well known as to help discrimination here. The interaction of sensory and motor nerves, under external stimulus alone, gives reflex action. To this must be added the phenomena of inhibition,—the restraints which a sensory system is capable of placing on its own activity. Next, we reckon the action of sense in stimulating appetite, which brooks no hindrance, marking one type of "the struggle for existence," of which Darwin has discoursed with fulness.

What we need to account for is the immense advance occurring in life when intelligence interposes to check impulse, and, after carrying through a course of reflection, orginates a higher motive, or changes the passion into actual motive. We need not here include the still greater complexity of action when moral considerations are introduced. We restrict the reference to self-interest.

The intervention of intelligence in the determination of action is an occurrence so different from all that can be traced, even by faintest anticipation, in lowest orders of life, that it involves a reversal of the law on which the hypothesis of evolution depends. Keeping in view here the difference between the lower unintelligent animals and the higher intelligent animals, it holds true for all of them, that the law of pleasure and pain rules their life. It is dominant in the lower as in the higher; but in the higher, passion

is fiercest, just as the muscular power is greatest, and the struggle for gratification is most violent.

When intelligence is in the ascendant in life, the whole position is changed, even to the extent of changing the law of activity. We have to adapt our theory to this transformation, and in consequence the conception of evolution is placed in the midst of a host of perplexities. The main question is this,—How can evolution of organism account for life capable of rational self-direction? Inhibition may supply suggestions here, but these avail only on the side of restraint; we need here to account for a power which places the whole life under a new order of government. When we include social relations, there are rival interests, consequent competitions, and new place and form for progress. All these are available for analogies. But the main puzzle is untouched,—out of organism to evolve the intelligence which first directs *its own* activity, and *then* directs organic activity.

This order—first intelligent action, second organic action—is essential to the case. No theory meets the requirements which does not undertake to explain this priority. Here, in contrast with what is observed in lower types of life, both unintelligent and intelligent, reflection shapes the purpose which the agent afterwards makes an effort to fulfil. This is something entirely new in the activity of life; this presents the hardest problem in natural history.

What is meant by Intelligence as concerned with action may be best indicated by pointing out what it does, for thus we command the facts and their history. Passion rises in man just as in the animals. There is not anything distinctive here. But in human life, the passion is restrained, reflection begins, and is prosecuted, while restraint over passion is maintained. There is comparison of consequences, contrast of the good and the better, formation of a conception, and next of a definite *purpose*, and only then is the sensori-motor system brought into action in relation to its environment. Whether the passion is then gratified, or is not gratified, does not affect our problem. In either case, the whole course of reflective procedure is carried through. The evolution of the power to accomplish this constitutes the great perplexity. How can we, through continuous action of the senses, reach the power we name Intellect, and that power over intellectual action which we see

to be implied in the ordinary conditions of its exercise. It does not seem to help us to suggest, as Professor Bain does, that feeling has an intellectual side and a volitional, for feeling neither thinks nor wills. What Cyples has well described as "congruent consecutive activity of associated sense-apparatus" does not lead us up to the facts to be explained. When we set receptivity and activity in contrast, we can agree with Mr Romanes in naming the contrasts recepts and concepts; but the laws of the recepts, as these are on the way to the concepts, imply something more than the "congruent consecutive activity of associated sense-apparatus." It does not appear that sense-apparatus is able to evolve the power which takes control of human activity in its ordinary but distinctive phases.

There is still greater complexity of procedure behind all this, when we include a representation of the higher government which we name moral life. I have not introduced this; there is not room for doing so in this paper, and there is already ample material before the Society for discussion.

Having regard to the conciseness of statement requisite in such a paper as this, I have endeavoured to supply illustrations sufficiently varied to admit of test in course of subsequent debate.

I close by stating the general position maintained. So far as human organism is concerned, there seem no overwhelming obstacles to be encountered by an evolution theory; but it seems impossible under such a theory to account for the appearance of *homo sapiens*, —the thinking, self-regulating life, distinctively human.

On Coral Reefs and other Carbonate of Lime Formations in Modern Seas. By John Murray, LL.D., Ph.D., and Robert Irvine, F.C.S.

(Read December 2, 1889.)

The vast organic accumulations known as coral reefs are, undoubtedly, among the most striking phenomena of tropical oceanic waters. The picturesque beauty of coral atolls and barrier reefs, with their shallow placid lagoons, and their wonderful submarine zoological and botanical gardens, fixed at once the attention of the

early voyagers into the seas of equatorial regions of the ocean. Questions connected with the peculiar form, the structure, the origin, and the distribution of these great natural productions have, from the very outset, puzzled and interested all those who delight in the study of natural things. In this communication we propose to point out and discuss some of the more general phenomena of oceanic deposits, with special reference to the functions of corals and other lime-secreting organisms, and the accumulation of their dead shells and skeletons on the floor of the great oceans.

Coral reefs are developed in greatest perfection in those ocean waters where the temperature is highest and the annual range is least. It may be said that reefs are never met with where the temperature of the surface water, at any time of the year, sinks below 70° Fahr., and where the annual range of temperature is greater than 12° Fahr. Bermuda, which is the coral island the farthest removed from the equator,* and one or two other outlying reefs, may be, in a sense, exceptions to this statement, for in these exceptional cases the temperature of the ocean water appears occasionally to fall to 66° or 64° Fahr., and there is a wider annual range than 12° Fahr. This condition of high temperature with small range in the temperature of the water is only to be met with in the middle and western portions of the Atlantic and Pacific Oceans and the central parts of the Indian Ocean; consequently, coral reefs flourish along the eastern shores of the continents where the coasts are bathed by currents of pure oceanic water coming directly from the open sea, while, on the other hand, they are absent along the western shores of the continents where the water is colder and the annual range is very much greater—for instance, off the western coasts of America and Africa. The "Challenger" observations have also shown that the layers of warm surface waters are much thicker towards the western parts of the great oceans; consequently, reef-forming organisms flourish at a greater depth along the eastern shores of the continents than in positions further to the eastward in the open ocean, where the warm layer of water—over 70° Fahr. —is much thinner. Throughout the temperate and polar regions there are no coral reefs. This is all the more remarkable, seeing that organisms belonging to the same orders, families, and even

* Lat. 32° N.

genera as those which build up coral reefs flourish throughout colder, and even in polar, seas. In these colder seas the representatives of the reef-builders either do not secrete carbonate of lime in their body-walls, or if they do so, the shells or skeletons are much less massive than in tropical waters. An attentive examination of the animals procured by the dredge and trawl from all depths shows that in descending into deeper water in equatorial regions the amount of carbonate of lime secreted by the animals living on the sea bottom becomes less with increasing depth, and all the calcareous structures of the organisms become less massive with the descent into the deeper and colder water of the abysmal regions. This remark does not, of course, apply to the shells and skeletons of surface organisms which have fallen to the bottom from the surface waters.

Still another illustration of the same fact is furnished by the study of the pelagic organisms collected in the surface and subsurface waters by means of the tow-nets. In the warmest tropical waters there are numerous species of Pteropoda, Heteropoda, Gasteropoda, Foraminifera, and Coccospheres and Rhabdospheres (calcareous Algæ), which lead a purely pelagic existence, and secrete carbonate of lime shells. Mr Murray estimates from his tow-net experiments that at least 16 tons of carbonate of lime exists in this form at any moment of time in a mass of tropical oceanic water one square mile in extent by 100 fathoms in depth.* The number of species and individuals of these lime-secreting organisms decreases, and the shells become less massive, with a wider removal from the equator and an approach to the colder water of the poles, till we find in the surface waters of the polar regions only one or two thin-shelled Pteropods, and one, or at most two, dwarfed species of pelagic Foraminifera. It would appear then that organisms, as a whole or individually, are able to, and actually do, secrete more lime in regions where there is a uniformly high temperature of the ocean water than in those regions where there are great seasonal fluctuations of temperature, or where there is a uniformly low temperature of the water, as in the polar regions and in the deep sea. In temperate seas more carbonate of lime is secreted in the warm

* Murray, "Structure and Origin of Coral Reefs," *Proc. Roy. Soc. Edin.*, vol. x. p. 508, 1880.

summer months than during winter months. Indeed, a high temperature of the sea water is more favourable to abundant secretion of carbonate of lime than high salinity.

An examination of the deep-sea deposits collected by the "Challenger" and other expeditions in all oceans shows that, after the death of the pelagic organisms above referred to, their calcareous shells are rained down on the ocean's bed, and there make up the larger part of the deposits known as Pteropod and Globigerina oozes, as well as a very considerable part of nearly all other marine deposits. If we take the samples of deep-sea deposits collected by the "Challenger" as a guide, the average percentage of carbonate of lime in the whole of the deposits covering the floor of the ocean is 36·83, and of this carbonate of lime, it is estimated that fully 90 per cent. is derived from the remains of pelagic organisms that have fallen from the surface waters, the remainder of the carbonate of lime having been secreted by organisms that live on or attached to the bottom. If coral muds and sands, together with Pteropod and Globigerina oozes, be considered, it is estimated that these contain an average percentage of 76·44 of carbonate of lime, and cover about 51,859,400 square miles of the sea bottom. We have little knowledge as to the thickness of these deposits, still such as we have goes to show that in these organic calcareous oozes and muds, we have a vast formation greatly exceeding in bulk and extent the coral reefs of tropical seas; they are most widely distri-

TABLE *showing the Estimated Area, Mean Depth, and Mean Percentage of* $CaCO_3$, *of the different Deposits.*

Deposit.		Area, square miles.	Mean Depth in fathoms.	Mean per cent. of $CaCO_3$.
Oceanic Oozes and Clay,	Red clay,	50,289,600	2727	6·70
	Radiolarian ooze,	2,790,400	2894	4·01
	Diatom ooze,	10,420,600	1477	22·96
	Globigerina ooze,	47,752,500	1996	64·53
	Pteropod ooze,	887,100	1118	79·26
Terrigenous Deposits,	Coral sands and muds,	3,219,800	710	86·41
	Other terrigenous deposits, blue muds, &c.,	27,899,300	1016	19·20

buted in equatorial regions, but some patches of Globigerina ooze are to be found even within the Arctic circle in the course of the Gulf Stream. The table on preceding page shows the estimated area of the various kinds of deposits, with the average depth, and average percentage of carbonate of lime in each.

One of the most remarkable facts discovered by the "Challenger" expedition is that, although the dead shells of these pelagic organisms are rained down on the sea bottom, and in shallower depths accumulate so as to form calcareous deposits of immense extent, still, in other contiguous but deeper areas, these shells do not accumulate on the bottom, being wholly removed either while falling through the water or shortly after reaching the ocean's floor. The pelagic organisms are as abundant in the surface waters over the one area as over the other, the only apparent difference in the conditions being one of depth. In the shallowest deposits of the open sea, shells representative of nearly all the lime-secreting surface organisms are to be found in the deposits. With increasing depth the more delicate ones disappear from the bottom till, in 1800 or 2000 fathoms, it is rare to find more than traces of Heteropod, Pteropod, or the more delicate pelagic Foraminifera, shells in the deposits, while these same delicate shells occasionally make up fully one-half of the carbonate of lime that is present in depths of 700 or 1000 fathoms. Again, in the still greater depths of 3000 and 4000 fathoms and deeper, the Foraminifera, Coccoliths, and Rhabdoliths are either wholly removed, or are represented only by the broken fragments of the thickest and most compact shells, like *Pulvinulina menardii*, *Sphæroidina dehiscens*, or *Globigerina conglobata*. This gradual decrease in the quantity of carbonate of lime in the deposits with increasing depth is well illustrated in the following table, showing the percentage of lime in the samples of deep-sea deposits collected by the "Challenger" towards the central parts of the ocean basins, away from the immediate influence of the débris from continental land or volcanic islands.

The organic oozes, including the red clays and the coral deposits, make up a total of 231 samples, and are arranged as follows, showing the percentage of carbonate of lime in relation to depth:—

14	cases	under 500	fathoms,	m. p.c.	86·04.
7	„	from 500 to 1000	„	„	66·86.
24	„	„ 1000 to 1500	„	„	70·87.
42	„	„ 1500 to 2000	„	„	69·55.
68	„	„ 2000 to 2500	„	„	46·73
65	„	„ 2500 to 3000	„	„	17·36
8	„	„ 3000 to 3500	„	„	0·88.
2	„	„ 3500 to 4000	„	„	0·00.
1	„	over 4000	„	„	trace.

The fourteen samples under 500 fathoms are chiefly coral muds and sands, and the seven samples from 500 to 1000 fathoms contain a considerable quantity of mineral particles from continents or volcanic islands. In all the depths greater than 1000 fathoms the carbonate of lime is mostly derived from the shells of pelagic organisms that have fallen from the surface waters, and it will be noticed that these wholly disappear from the greater depths. These figures are derived from a study of the "Challenger" deposits alone, but they are confirmed, as to the general result, by an examination of the deposits collected by the U.S.SS. "Tuscarora" and "Blake," by H.M.SS. "Egeria" and "Investigator," the ships of the Telegraph Construction and Silvertown Companies, and other ships. One other peculiarity as to the distribution of carbonate of lime organisms on the ocean's floor may be noted. Where these calcareous shells are most abundant on the surface, as in the tropics, the remains of the dead shells are as a rule found at greater depths on the bottom than in temperate or polar regions, where they are relatively much less abundant in the surface waters.

In his paper on the Origin of Coral Reefs, published many years ago, Mr Murray pointed out that sea water, rushing in and out of the lagoon twice in the twenty-four hours, would take up and carry away large quantities of the carbonate of lime which, in the form of coral sand and mud, covers the bottom of these shallow basins. Just as the surface shells are dissolved by falling through the layers of ocean water, so in this case the dead coral fragments are dissolved by the sea water that continually passes over them; in this way chiefly he accounted for the formation of lagoons in atolls and barrier reefs.

During the past few years a large number of experiments have been carried on at the Scottish Marine Station for Scientific Research, with the view of throwing some additional light on the oceanic phenomena referred to in the preceding paragraphs, in so far as these relate to the secretion and solution of carbonate of lime under varying conditions. Those dealing with the secretion of carbonate of lime by organisms will be considered in the first place, and afterwards those treating of the solution of the dead carbonate of lime shells and skeletons will be discussed.

A detailed account of some of the experiments will show the nature of the investigations, and indicate the results that have been obtained in so far as they bear on the subject with which we are here dealing.

Experiment I. A number of hens that were laying eggs regularly were placed in a wooden building where they could not pick up substances containing lime salts, except such as were supplied with their food. At first they were not given any lime with their food, and their drink was distilled water; in a few days they laid eggs with only a membranous covering. Sulphate of lime was then given with the food, and in the course of a few days they commenced to lay eggs with the usual carbonate of lime shells. When the sulphate of lime was stopped, the eggs again became membranous and shelless. In this way it was shown that hens were able to provide their eggs with the usual carbonate of lime shells from the silicate, sulphate, nitrate, phosphate, and carbonate of lime that was administered with their food. Further, it was found that they could not make use of the magnesium and strontium salts for the purpose of forming shells for their eggs.* It is believed that in the case of the various lime salts the lime passes through the blood in the form of phosphate to the point of secretion, where it is deposited as carbonate of lime.

Experiment II. An artificial sea water was prepared, from which carbonate of lime was rigidly excluded, and in this some living crabs were placed. They lived for many months, and after ecdysis produced the usual exo-skeleton of carbonate of lime from the lime salts, other than carbonate, present in the water.†

* See Appendix, Table III.

† See Irvine and Woodhead, *Proc. Roy. Soc. Edin.*, vol. xv. p. 308, 1888.

Experiment III. An artificial sea water similar to the above (II.), absolutely free from carbonate of lime, was neutral at first, but after the introduction of living crabs became at first slightly acid, and then, after a time, distinctly alkaline in character. This was found to be due to uric acid, urea, and other effete products thrown into the water by the crabs, and to their subsequent decomposition, with the formation of carbonate of ammonia, and ultimately of carbonate of lime. In one of the tanks the alkalinity was equal to the production of about 45·36 grammes of carbonate of lime—this representing the amount of that body formed in twelve months through the agency of four small crabs, weighing in all about 90·72 grammes, and whose exo-skeletons contained only about 5·184 grammes of carbonate of lime.

Experiment IV. The following experiments were conducted with the view of throwing some light on the above changes. Three litres of sea water and 750 c.c. of urine were mixed and kept exposed to the air at a temperature ranging from 60° to 80° Fahr. What was lost by evaporation was made up by the addition of pure water. This solution was at first acid; after a few days it became neutral, and later, as decomposition of the urine advanced, it became strongly alkaline. On heating a portion of the solution, ammonia was given off, so that the alkalinity was evidently due to the formation of ammonia. After seven days a bulky flocculent precipitate was thrown down, which on analysis consisted of organic matter, double phosphate of magnesia and ammonia, together with a small quantity of carbonate of lime, as shown by the following analysis:—

Precipitate after Seven Days.

Water and organic matter containing ammonia, 7·38 grains,	31·81
Carbonate of lime,	4·85
Phosphate of magnesia and ammonia,	51·10
Phosphate of lime,	12·24
	100·00

After other ten days, during which time the liquid, filtered from the above precipitate, was exposed under the same conditions, a further precipitate was thrown down, differing from the first in that

it consisted principally of carbonate of lime, as shown by the following analysis:—

Precipitate Ten Days after the first one above noted.

Water and organic matter,	20·25
Carbonate of lime,	75·35
Carbonate of magnesia,	1·02
Phosphate of magnesia,	3·38
Ammonia,	(none)
	100·00

Examined with the microscope, the precipitate was seen to consist of the characteristic crystalline forms of carbonate of lime. It effervesced freely with acids. The first precipitate (seven days) weighed 2·37 grammes; the second (seventeen days), 2·75 grammes—that is, 5·12 grammes in all. Thus after adding the carbonate of lime in solution, present in the filtrate, there is practically enough lime in these precipitates to account for all the sulphate and carbonate of lime present in the sea water used. In other words, this reaction had removed all the soluble calcium salts from the sea water, principally as more or less insoluble crystalline carbonate.

Experiment V. An experiment was made with a solution of sulphate of lime of the same strength as present in sea water. 500 c.c. of this solution was mixed with 100 c.c. of urine under the same conditions as in the previous experiment (IV.); after eleven days the precipitate was collected, and was found to contain 51 per cent. of carbonate of lime. This, after adding the carbonate of lime in solution in the filtrate, accounted for all the lime present in the original solution of sulphate of lime made use of.

Experiment VI. In this experiment an attempt was made to imitate the conditions occurring in nature as nearly as possible. Nine small crabs, weighing in all 11 ounces, were placed in a shallow glass vessel containing 2 litres of ordinary sea water, and were fed on mussel flesh. The water was never renewed nor aerated, the effete matters passing into it. At the end of fourteen days all the crabs had died, and were removed. The water being then in a putrid condition, it was set aside for about three weeks at a temperature ranging from 70° to 80° Fahr. All the conditions of the last two experiments

(IV. and V.) were observed, and it was found that crystals of carbonate of lime had been thrown down in amount practically equivalent to all the calcium present in the sea water employed.

Experiment VII. We obtained the liquor from a number of living oysters, and examined it before decomposition had begun.

It appeared to be a mixture of lymph with unchanged sea water; the specific gravity at 60° F. was 1·023, the amount of chlorine per litre was 17·56 grammes, indicating a considerable admixture of fresh or river water.

The total lime in a litre of this liquor was .	0·7205	grammes,
whilst the total lime in ordinary sea water of the same sp. gr. only amounts to .	0·5316	"
giving an excess of total lime, .	0·1889	"
The alkalinity of the oyster liquor amounted to of carbonate of lime per litre,	0·3675	"
whilst the alkalinity of sea water of the same sp. gr. amounts to	0·1094	"
showing an increase of alkalinity equivalent to	0·2581	"
per litre.		

We have thus an accumulation of lime (in excess over that present in sea water) amounting to 0·1889 grammes per litre, the greater part of which is in the form of carbonate in solution, presumably in the amorphous or hydrated condition.

Any doubts as to how this excess of carbonate of lime comes to be present is set at rest by the fact that the fresh and absolutely undecomposed liquor containing this excess of lime contains also (saline) ammoniacal salts equal to 18 parts per million, or about sixty times that present in any sea waters we have examined.

Experiment VIII. A similar experiment was made with the liquor taken from living mussels (from the mussel beds at Granton), the results coinciding with those obtained in Ex. VII.*

* Theoretically, urea plus two molecules of water give carbonate of ammonia. If, therefore, carbonate of ammonia be a stage in the formation of urea, it is not unnatural to suppose that, in shell-forming animals, the shell formation may be the stage without any formation of urea. For special method for the determination of saline ammonia in sea water, see page 101, Appendix.

Experiment IX. Albumen taken from a freshly-laid egg was diluted with water and pure potash added, and the clear solution was Nesslerised. This showed no trace of ammoniacal salts.

These experiments show the alteration that is produced in the constitution of sea salts, and especially in the constitution of the lime salts, by the effete matters thrown into the sea by animals; in the case of Experiment III., the effete matter from four small crabs was sufficient to produce, in twelve months, a quantity of carbonate of lime equal to nearly five times the weight of their own calcareous structures. Wherever effete animal matters are being thrown into the sea, or wherever animal structures are undergoing decay in the ocean, decomposition products, many of them of a complex constitution, pass into solution. These, in the presence of the sea water salts, give rise to many reactions, and, among others, the formation of ammoniacal salts always takes place to a greater or less extent. Sea water collected among the coral atolls of the Louisiade Archipelago * contained in one million parts—

Saline ammonia, ...	0·48
Albuminoid ammonia, ...	0·18
	0·66

Water collected by the "Challenger" in the North Atlantic, lat. 30° 20′ N., long. 36° 6′ W., contained of—

Saline ammonia, ...	0·26
Albuminoid ammonia, ...	0·16
	0·42

Water from the German Ocean near land contained of—

Saline ammonia, ...	0·13
Albuminoid ammonia, ...	0·13
	0·26

So that water from the coral reef regions contained nearly twice as much of these ammoniacal salts as water from the North Atlantic, and nearly three times as much as water from the German Ocean. This shows, it appears to us, that the carbonate of ammonia,

* These samples were received through Captain Wharton, F.R.S., Hydrographer to the Admiralty.

arising from the decomposition of animal products, in presence of the sulphate of lime of sea water, becomes carbonate of lime and sulphate of ammonia.* Thus the whole of the lime salts in sea water may, in these circumstances, be changed by this reaction into carbonate, and may in this way be presented to the coral and shell builders in a form suitable for their requirements. The temperature of the water is of great importance in this reaction. In cold water, of which the great bulk of the ocean consists, the decomposition of nitrogenous organic matter is greatly retarded, whereas, in tropical surface waters, it proceeds with great rapidity. Here, then, we have probably the explanation of the great development of the massive structures formed by lime-secreting organisms in the coral reef regions, which, as has been pointed out, are also the regions of highest and most uniform temperature in the ocean. In the same way we may account for the great extension of lime-secreting pelagic organisms in the tropical surface currents that flow north and south from the equator. Thus coral reef-builders and pelagic organisms may not only benefit by the decomposition products arising from their own effete matters, but also from the undecomposed nitrogenous matter carried to equatorial regions from the cold water of the deep sea or from polar regions

The quantity of carbonate of lime normally in sea water is exceedingly small, and the opinion hitherto held seems to have been that lime-secreting organisms had to pump enormous quantities of sea water through their bodies in order to be able to separate out a sufficient quantity to form their shells and skeletons.† It seems

* The sulphate of ammonia is in turn absorbed by the marine flora which forms the food of the marine fauna, and is in part resolved into nitrates and free nitrogen.

† Bischof, *Chem. and Phys. Geol.*, vol. i. p. 180, says:—"In order to form a conception of what testacea are capable of effecting by organic agency, I determined the weight of ten oysters and their shells. After they had been opened the enclosed sea water was, as far as possible, removed. The weight of the shells varied from 2·78 to 7·57 that of the oysters. . . . .

"No one can doubt that it was the carbonate of lime dissolved in the sea water which *alone* furnished the material for the formation of these shells.

"If now we assume that the sea water contains $\frac{1}{10000}$ of carbonate of lime, and that the oysters are capable of deriving all their calcareous substance from the water by organic agency, it follows that the above number of oysters required for the formation of their shells 345 to 587 lbs., or 5·2 to 8·9 cubic feet of sea water.

"This quantity is from 27,760 to 75,714 times the weight of their shells.

much more probable that the reactions indicated by the above experiments render the whole of the lime salts in the sea water, and especially the sulphate, available for the coral polyps to construct their massive structures. In higher animals, like hens, the carbonate of lime is secreted from the blood; but in coral polyps, in which there is no true circulatory system, and where the animal is immersed in the sea water, it is most probable that the reaction above referred to—the formation of carbonate of ammonia—is in every way advantageous to these lime-secreting organisms, and facilitates the deposition of carbonate of lime by the protoplasm. In the case of all the lower classes of lime-secreting organisms this change in the constitution of the lime salts may take place within the tissues of the animals. In the case of the oysters (Exp. VII.) the excess of carbonate of lime observed in the liquor or diluted lymph was clearly due to the decomposition of the sulphate of lime in the sea water by carbonate of ammonia secreted as such by the protoplasm of the animal.

The quantity of salts in a given volume of sea water varies with the position from which the sample is collected, and according as the salinity is high or low; but it has been shown, by hundreds of analyses from all parts of the ocean, that the actual ratio of acids and bases—that is, the ratio of the constituents of sea salts—is constant in waters from all regions and depths of the ocean, with one very significant exception, namely, that of lime, which is present in slightly greater proportion in water from the greater depths. In the ordinary analyses, however, the rarer elements are never determined, although it is known that there are traces of nearly every element in sea water. Theoretically, every base may be combined with every acid, and the whole solution must be in a continual state of flux as to its internal constitution. An

"According to these results, an oyster would appear to be a pumping-machine of extraordinary activity. . . . .

"It is also known that in the testacea there is a continual current of water from behind forwards within the mantle. . . . .

"This current of water in the oysters appears to be astonishing when we compare it with the quantity of fluid which passes through the human body.

"When a man weighing 150 lbs. consumes even 5 lbs. of liquid daily during a period of seventy-five years, still a quantity of liquid, only 912·5 times the weight of his body, would pass through his organism, or only about $\frac{1}{30}$ to $\frac{1}{35}$ of the sea water which has passed through the oysters."

indication as to the nature of these internal changes is given in the above experiments, dealing chiefly with the reactions brought about in sea water by the decomposition of the effete products thrown into it by animals and by the decay of organic tissues. Through the action of the carbonate of ammonia on the sulphate of lime in the sea water, it is evident that there is a production of a large quantity of carbonate of lime in a form easily available for lime-secreting organisms. In the laboratory, when carbonate of ammonia is added to sea water nine-tenths of the calcium in solution is thrown out as carbonate of lime, while the magnesian salts remain in solution; so that if the reaction above indicated be that which takes place in the ocean, then to this circumstance may be due the fact that carbonate of magnesia is almost wholly absent from recent coral reefs and deep-sea calcareous formations. The above experiments appear clearly to show that the alkalinity of sea water is due to the presence of carbonate of lime in solution; for, in addition to the fact that this body is thrown down from the decomposing solutions after these become alkaline, we find that when carbonate of lime is added to a neutral artificially-prepared sea water, absolutely free from carbonate of lime, it at once gives the alkaline reaction common to normal sea water.

That the amount of the nitrogenous organic matter in a state of suspension and solution in the ocean must be enormous, will appear evident when it is remembered that the floor of the ocean throughout its whole extent is covered with living animals, that the surface and sub-surface waters and shallow depths off all coasts are crowded with plants and animals down to a depth of several hundred fathoms, and that the "Challenger" experiments have shown that some species of animals flourish in all intermediate depths in ocean water from the surface down to the very bottom. The waste products arising from the functional activity of these organisms, and the nitrogenous organic products arising from the decomposition of their dead bodies, must work continual changes on the internal constitution of sea-water salts, varying according to their amount, the temperature, sunlight, and other conditions. It has been shown that ammoniacal salts are to be found everywhere in the ocean, but much more abundantly in the warm tropical waters than in the polar seas—a result due to the rapid decomposition of nitrogenous

organic matters at a high temperature, and its retardation in colder waters. The ammonia of the air and all the substances carried into the sea by the drainage from the land also effect changes in the internal constitution of the sea water; indeed, the peculiar pelagic fauna and flora, which are met with in all regions of the ocean where the ocean is affected by river and coast waters, are as much in relation with the internal constitution of the sea-water salts as with the lower salinity which prevails in these circumstances. It is well known that organic substances in the presence of alkaline and earthy sulphates become oxidised at the expense of the oxygen of these salts, with the production of carbonic acid and sulphuretted hydrogen,* which on oxidation produces sulphuric acid. The whole of the organic carbon, which it has been pointed out is of enormous amount, must apparently be oxidised into an equivalent amount of carbonic acid, and further of sulphuric acid. The effects of this reaction are likely to be more marked in the deeper parts of the ocean, where all motions of the sea water must be extremely slow, and where consequently the effete products tend to accumulate. In this way the larger amount of lime and carbonic acid and the less amount of oxygen in deeper waters are to be accounted for. Not only so, but the very existence of such a relatively large quantity of sulphate of lime in sea water goes far to prove that this reaction is continually taking place, seeing that sulphuric acid cannot exist in a free state in the presence of carbonate of lime. Thus it is probable that the quantity of sulphate of lime in solution in the ocean is limited only by the amount of organic decomposition that takes place in ocean waters. On the other hand, if marine organisms procure the whole of their carbonate of lime from the sulphate of lime by the reaction of the ammoniacal salts, then the amount of lime that may be secreted from ocean waters is likewise limited by the amount of organic matter undergoing this oxidation process in the ocean.†

* $MSO_4 + 2C = 2CO_2 + MS$
$MS + CO_2 + H_2O = MCO_3 + H_2S$
$H_2S + 2O_2 = H_2SO_4$.

† Gmelin, *Chemistry*, vol. ii. p. 191, states:—"In hot climates, as on the west coast of Africa, where the water of the rivers highly charged with organic matter, mixes with the sea water which contains salts of sulphuric acid [sulphates], the same decomposition takes place, extending sometimes to a

From the foregoing discussion it appears evident that in the various reactions taking place continually in ocean waters there are special conditions which are favourable to the secretion of carbonate of lime by organisms, chief among these being a high and uniform temperature of the water at all seasons of the year.

If now we turn our attention to the solution of the dead carbonate of lime shells and skeletons by the action of sea water it will, in like manner, be found that the rate of this solution varies greatly according to the conditions in which these dead remains are exposed to the solvent power of the water.

The tables at the end of this paper give some of the results of a large number of experiments that have been conducted on the solubility of carbonate of lime in different states and in different samples of sea water. It may be pointed out that the normal amount of carbonate of lime dissolved in sea water is very small, especially when compared with the large amount of this substance that is continually being secreted from the sea by organisms. The amount in ordinary sea water (sp. gr. 1·026) is 0·12 grms. per litre. Sea water may, however, take up 0·649 grms. per litre of carbonate of lime in the amorphous or hydrated condition. This solution is then supersaturated, and on being allowed to stand, the carbonate of lime is thrown out of solution in the crystalline form, and after a time the solution may contain less than is normally present in sea water. It has been found that sea water can hold in solution (at 80° F.) for at least a week 0·16 grms. per litre more than is normally present in it, and artificially prepared sea water can also hold in solution 0·28 grms. per litre for at least a week, and 0·12 grms. (the normal amount in ordinary sea water) for six weeks.

It thus seems that although sea water under certain conditions may take up a considerable quantity of carbonate of lime in solution, yet it is unable permanently to retain in solution more than is usually found to be present in sea water, and it is owing to this that

distance of 27 miles from the mouths of the rivers, the water containing hydrosulphuric acid, sometimes as much as six cubic inches to a gallon."

This is confirmed from samples of water, which we have examined, taken from the roadstead at Monte Video by the telegraph ship "Seine." It consists of a mixture of fresh and salt water, with a considerable amount of organic matter which has decomposed the sulphate, giving rise to a large amount of hydrosulphuric acid.

the amount of carbonate of lime is so constantly low. The reaction between organic matter and the sulphates present in sea water, to which we have referred, tends also to keep the amount of carbonate of lime in solution at about one-half (0·12 grms.) of what it might contain (0·28 grms. per litre). This peculiarity of sea water, in taking up a large amount of amorphous carbonate of lime and throwing it out in the crystalline form accounts for the filling up of the interstices of massive corals with crystalline carbonate in coral islands and other calcareous formations, so that all traces may ultimately be lost of the original organic structure.

Our experiments show that there is very great diversity as to the amount of carbonate of lime that will pass into solution in sea-water from various calcareous structures in a given time. As a rule, the more definitely crystalline the substance the less is it soluble. Crystalline calcspar is less soluble than massive varieties of coral, and these again less so than the more porous varieties. To a certain extent, this variation may be accounted for by the amount of surface exposed to the solvent action of the water, which is much greater in porous coral than in calcspar; but it is found that finely-powdered calcspar is less soluble than finely-powdered coral, and we have already noted that the amorphous or hydrated carbonate of lime is much more soluble than any other form of the substance. In shells and corals those layers which have been recently deposited by the animals are more rapidly removed in solution than those of older date, which have, in consequence, assumed a more crystalline structure. The rate of solution is also much greater when the water is constantly renewed than when the same water remains in contact with the coral, and the solution approaches to saturation. It appears, then, that those portions of a coral or shell that have been recently in direct life-connection with the organism are more soluble than those portions which are older, and have consequently largely passed into the crystalline condition.

In conducting these various experiments with carbonate of lime, we were early led to suspect that different samples of sea water possessed very different solvent powers, and this was confirmed by special observations. For instance, water taken last summer (1889) from the German Ocean, 30 miles outside the Island of May, had a very distinct solvent power on the corals exposed to its action, while

water taken in January 1890 from the same spot, and having a slightly lower specific gravity, exerted little or no solvent action on the same corals. These remarks also apply to waters taken from near shore at North Berwick and Granton in the summer of 1889 and January 1890. The lower specific gravity of the winter waters may be regarded as, to some extent, reducing the solvent power; but this is more probably to be attributed to the absence of free carbonic acid, that is, carbonic acid in excess of what is required to saturate the free base in the sea water as normal carbonate ($CaCO_3$). To test this point, carbonic acid was added to one of these winter waters, but in quantity not sufficient to destroy its alkaline character. Powdered carbonate of lime was then added to this water, as well as to another portion of the water to which carbonic acid had not been added, and both samples were allowed to remain under exactly the same conditions for fourteen hours. It was found at the end of this time that the water to which no carbonic acid had been added had not taken up any of the carbonate of lime, while the water to which carbonic acid had been added, under the conditions above stated, had dissolved an appreciable amount.

These results appear to indicate that there is more carbonic acid in summer than in the winter waters in our latitude, due probably to the increased activity of animal life, and although it may be insufficient to form bicarbonate with the free base present, such waters have a distinctly more powerful action on carbonate of lime structures, the greater the quantity of carbonic acid they contain, and especially so when the quantity is more than would form a sesqui-carbonate.

Mr Buchanan's observations on board the "Challenger" showed that the carbonic acid present in sea water, over and above that necessary to form normal carbonate of lime, was subject to great variations.* In a few instances it was more than sufficient to form fully saturated bicarbonate, but in the majority of cases it fell somewhat short of that amount, supposing that this surplus carbonic acid does really form a bicarbonate with the surplus base. In any case our experiments have shown that alkaline sea water will dissolve up carbonate of lime in addition to what it already contains, and that the rapidity with which this is effected depends on the

* See Dittmar, *Phys. Chem. Chall. Exp.*, Part I.

excess of carbonic acid in the sea water over that necessary to form a normal carbonate, but especially over that required to form a sesqui-carbonate with the surplus base. It also appears that this is a much more effective agent in the removal of carbonate of lime shells than the solvent power of sea water itself, although, as we have already shown, sea water from which carbonic acid is absent can dissolve carbonate of lime in addition to what it normally contains, still the quantity that can be thus taken up is relatively small. Mr Buchanan's observations have also shown that carbonic acid is, as a rule, more abundant in bottom and intermediate waters than in surface waters, although in some places surface waters contain a very large quantity, as, for instance, among the coral islands of the Fijis. A series of experiments have recently been conducted which show that carbonated waters under high pressure will take up more carbonate of lime than when at the normal of atmospheric pressure.*

These results may now be applied to the explanation of the removal of carbonate of lime shells and structures from the deep sea deposits, and from the lagoons of coral islands. The fact that carbonic acid is more abundant in the deeper waters of the ocean is evidently connected with the respiration and decay of the organisms that live on the sea bottom, and with the decay of those that fall down to the bottom from the surface of the sea. The water that fills the deeper hollows has also in its passage to the equator passed over thousands of square miles of the sea floor covered with living animals. As this deep water has a very slow motion and is but slowly renewed, we would expect an accumulation of carbonic acid, and a deficiency of oxygen in these abysmal depths.

When, therefore, a carbonate of lime-secreting organism dies in the surface waters and the dead body commences to fall towards the bottom, its shell is at once exposed to solution from the action of the sea water and the carbonic acid, produced, it may be, by the decomposition of its own body. If the shell be an exceedingly thin one, as in the case of Heteropods and some Pteropods, it may be wholly removed before reaching the bottom, at the depth of a few hundred fathoms. The great majority of shells, however, are only partially removed during the first few hundred fathoms, and on reaching the bottom at these lesser depths they accumulate

* Reid, *Proc. Roy. Soc. Edin.*, vol. xv. pp. 151–157, 1888; Appendix, p. 108.

there. After reaching the bottom more of the shell may be dissolved, but in these positions there are circumstances which retard solution. Where shells reach the bottom in depths of a few hundred fathoms, they are soon covered up by other shells, and are surrounded by sea water which is completely or nearly saturated with carbonate of lime, from its being but slowly renewed, and from having been for a long time in contact with a wide extent of calcareous deposits at the bottom of the sea. It is probable that, say, at a depth of 500 fathoms, fully 80 per cent. of the carbonate of lime shells that fall from the surface reach the bottom and accumulate there.

At depths, however, of 1500 or 2000 fathoms not more than 50 per cent. of these shells reach the bottom in a partially dissolved condition; moreover, the accumulation is here slower, from a greater number of shells being dissolved in their fall through the water, and after reaching the bottom the shell will be for a longer time exposed to the action of water before being covered up and protected by the fall of other shells, as is the case in shallower depths. In the greatest depths of the ocean all the surface shells appear to be removed before they reach the bottom, except probably some of the heavier and more compact ones, which lie there uncovered by other shells till wholly removed by solution. The water in these red-clay areas would, besides, take up the lime more readily, as there it is not in contact with great carbonate of lime deposits, like the water overlying a Globigerina or Pteropod ooze. These deeper layers of water are more active in the removal of carbonate of lime than the surface waters, not only because of the larger amount of carbonic acid they contain, and to the deoxidation of alkaline sulphates by organic matter giving rise to hydrosulphuric acid, but also because of the greater pressure, and the fact that, the substance of the shells being less compressible than water, they would fall more slowly, and hence would be longer exposed to the action of the deeper layers of water than to the action of those nearer the surface.

In this way we appear to have a perfectly rational explanation of the partial disappearance of carbonate of lime shells from the shallower depths, and their total disappearance from all the greater depths of the ocean. It is to be observed that all those shells in

which a considerable quantity of organic tissue is associated with the carbonate of lime, disappear in solution more rapidly than the shells of the Foraminifera, which contain little organic matter. During the whole of the "Challenger" cruise only two bones of fishes, other than the otoliths and the teeth, were dredged from the deposits, and all traces of the cetacean bones were removed, except the dense earbones and dense Ziphioid beaks. The remains of crustacean animals were almost wholly absent from deep-sea deposits, with the exception of Ostracode shells and the hard tips of some claws of crabs.

Turning now to the lagoons and lagoon channels of coral islands, it is believed that large quantities of carbonate of lime are in the same way being dissolved from these shallow basins as well as from the deposits of the deep sea, but under somewhat different circumstances. In the case of a shell falling to the bottom of the sea, it is continually brought in contact with new layers of water, which has the same effect as if a continuous stream of water were passing over the shell. In the case of the lagoons this last is what takes place. The water which flows in and out of the lagoons twice in twenty-four hours passes over great beds of growing coral, and from all the observations we have is largely charged with carbonic acid, owing probably to the large number of living animals on the outer reef over which the water passes on its way to the lagoon. This water passes continually over the dead coral and sand of the lagoon, and takes up and removes large quantities of carbonate of lime in solution (as well as suspension) for in these lagoons the spaces covered by dead coral débris always greatly exceed the patches of growing coral. Owing to the fact that the water of the lagoon is continually in motion, and constantly renewed, the layer in contact with the bottom of coral sand can never become saturated or unable to take up more lime, as is apparently the case in the layers of water in contact with the Globigerina ooze and other calcareous deep-water deposits.

From the foregoing discussion and observations it is evident that a very large quantity of carbonate of lime is in a continual state of flux in the ocean, now existing in the form of shells and corals, but after the death of the animals passing slowly into solution, to go again through the same cycle.

On the whole, however, the quantity of carbonate of lime that is secreted by animals must exceed what is re-dissolved by the action of sea water, and at the present time there is a vast accumulation of carbonate of lime going on in the ocean. It has been the same in the past, for with a few insignificant exceptions all the carbonate of lime in the geological series of rocks has been secreted from sea water, and owes its origin to organisms in the same way as the carbon of the carboniferous formations; the extent of these deposits appears to have increased from the earliest down to the present geological period.

At the present time most of the carbonate of lime carried to the ocean by rivers has been directly derived from calcareous stratified rocks formed by organic agency in the sea in earlier geological ages, but the calcium in these formations was in the first instance derived from the decomposition of the lime-bearing silicates of the earth's original crust, and this decomposition, which is still going on in the sea and on the land surfaces, is a continuous source of carbonate of lime.

In considering the analyses showing the average composition of sea salts, one is struck with the relatively small quantity of those very substances which are extracted so largely from sea water by plants and animals, viz., carbonate of lime and silica. Siliceous deposits are of vast extent, yet silica occurs merely in traces in sea water; carbonate of lime deposits are of vastly greater magnitude, yet carbonate of lime makes up only $\frac{1}{130}$th part of the saline constituents of sea water, and only $\frac{1}{8300}$th part of the whole bulk of sea water. Sulphate of lime is ten times more abundant than the carbonate in sea water; on the other hand, the river water that is poured into the ocean contains about ten times as much carbonate as it does of sulphate of lime.*

The total amount of calcium in a cubic mile of sea water is estimated from analyses to be 1,941,000 tons, and the total amount of calcium in the whole ocean is calculated at 628,340,000,000,000 tons. The total amount of calcium in a cubic mile of river water is estimated at 141,917 tons, and the total amount of this element carried into the ocean from all the rivers of the globe annually is

* Murray, "Total Annual Rainfall of the Globe," *Scottish Geog. Magazine*, vol. iii. p. 65, 1887.

estimated at 925,866,500 tons. At this rate it would take 680,000 years for the river drainage from the land to carry down an amount of calcium equal to that at present existing in solution in the whole ocean. Again, taking the "Challenger" deposits as a guide, the amount of calcium in these deposits, if they be 22 feet thick, is equal to the total amount of calcium in solution in the whole ocean at the present time. It follows from this that if the salinity of the ocean has remained the same as at present during the whole of this period, then it has taken about 680,000 years for the deposits of the above thickness, or containing calcium in amount equal to that at present in solution in the ocean, to have accumulated on the floor of the ocean. From the data here furnished a number of other interesting speculations might be indulged in, relating to the amount of carbonic acid that has been abstracted from the atmosphere and fixed in carbonate of lime deposits; the total amount of disintegration of lime-bearing siliceous rocks measured in terms of the calcium at present existing in solution in water and fixed in calcareous deposits; the relative proportions of substances secreted from the ocean, as compared with other materials derived from the direct disintegration of the land, forming deep-sea deposits; and the apparent accumulation of carbonate of lime formations towards the equatorial regions of the globe. These various matters will, however, be discussed in another place.

During the course of these investigations at the Scottish Marine Station, the following gentlemen have assisted us in carrying on the experiments referred to in this communication:—Dr G. S. Woodhead, Messrs G. Brook, W. S. Anderson, J. G. Ross, B.Sc., G. Young, A. Drysdale, and W. G. Reid.

APPENDIX.

Modification of the Method for the Determination of Ammonium Salts in Sea Water.

The method usually followed in water analyses, by which saline ammoniacal salts are distinguished from albuminoid nitrogenous matter, in so far as the process of distilling the sea water under examination, first with pure magnesia to eliminate the ammonia actually present, and in the subsequent treatment of the residual water with solutions of potash and permanganate of potash, being

very tedious and doubtful as to results, we devised and adopted the following process, which was found to answer exceedingly well. Pure potash was added to a measured and carefully filtered portion of the sea water, and the precipitate formed removed by filtration through filter-paper, from which any traces of ammonia (generally present in filter-paper) had been removed by washing with pure potash water, the clear filtrate was then Nesslerised in the usual manner. We found that by adopting this plan we had a speedy and accurate means of determining between the actual ammoniacal salts and the nitrogenous matter, both of which are as a rule present in sea water according to the proportion it carries of living or dead organisms.

Exp. IX. (p. 89) seems to prove in a conclusive manner that the addition of pure potash to a fluid containing albuminoids alone does not at once give rise to the production of saline ammonia.

TABLE I. *Showing the Average Composition of Sea Water Salts* (Dittmar).

Chloride of sodium,	77·758	77·758
Chloride of magnesium,	10·878	
Sulphate of ,,	4·737	15·832
Bromide of ,,	·217	
Sulphate of potash,	2·465	2·465
Carbonate of lime,	·345	3·945
Sulphate of ,,	3·600	
	100·000	100·000

TABLE II. *Showing the Composition of Sea Water* (Dittmar).

Analyses of Sea Water in 10,270 parts, or 1 litre, density ${}_4S_{15·56}$.

Water,	9897·073
Chloride of sodium,	289·980
Chloride of magnesium,	40·568
Sulphate of magnesia,	17·665
Bromide of magnesium,	0·809
Sulphate of lime,	13·425
Sulphate of potash,	9·193
Carbonate of lime,	1·287
	10,270·000

Table III. *Egg Shell (Hen's), showing the Effect of various Salts added to Food of Hens* (Young).

Dried at 212° Fahr. Membrane removed.

	Salts added to Food.					
	No. 1. Sulphate of Lime.	No. 2. Phosphate of Lime.	No. 3. Silicate of Lime.	No. 4. Nitrate of Lime.	No. 5. Carbonate of Magnesia.	No. 6. Carbonate of Strontia.
Hens laid Eggs.	Normal.	Normal.	Normal.	Normal.	Shelless.	Shelless.
Analysis of Shell.						
Organic matter, .	6·86	5·77	About same as in 1 and 2.	Same as in 1 and 2.	...	...
Carbonate of lime,	92·03	93·07			...	...
Phosphate of lime,	1·11	1·16			...	...
Sulphate of lime,	trace	trace			...	...
Silica, . .	,,	,,			...	...
Magnesia, . .	,,	,,			...	...
Iron, . . .	,,	,,			...	...

Table IV. *Showing the Composition of Artificial Sea Waters compared with ordinary Sea Water* (Young).

	Artificial Sea Waters.				Ordinary Sea Water.
	No. 1.	No. 2.	No. 3.	No. 4.	
Chloride of sodium, . .	2·7205	2·4804	2·4804	2·6996	2·7254
Chloride of magnesium, .	0·3794	0·3320	0·3320	0·3696	0·3813
Sulphate of magnesium, .	0·1551	0·1357	0·1357	0·1491	0·1660
Bromide of magnesium, .	...	...	0·0079	...	0·0076
Sulphate of lime, . .	0·1276	0·1116	0·1116	...	0·1262
Sulphate of potash, . .	0·1026	0·0898	0·0898	0·1125	0·0863
Chloride of calcium, . .	...	0·0908	0·0903	0·0964	...
Carbonate of lime, . .	...	...	...	...	0·0120
	3·4852	3·2398	3·2477	3·4272	3·5040
Water, ...	96·5148	96·7620	96·7523	96·5728	96·4960

TABLE V. *Showing Composition of Ecdysed Crab compared with ordinary Shore Crab* (Anderson).

Analysis of "Ecdysed" Crab which had lived ten days after casting in No. 2 Water, and of an ordinary Crab to compare with it. Size across the carapace, 1½ inches.

	Ecdysed Crab.	Shore Crab.
Chitinous matter in exo-skeleton, . .	3·77	10·76 grains.
Carbonate of lime in exo-skeleton, . .	5·50*	39·78 ,,
Carbonate of lime of interior, including stomachical teeth, &c., ...	0·25	1·53 ,,
Teeth (mandibles), ...	0·041	0·59 ,,
Phosphate of lime in exo-skeleton, . .	2·396	4·48 ,,
Phosphate of lime of flesh, lymph, and total interior structure, ...	0·18	0·39 ,,
Water, flesh, &c., ...	350·00	350·00 ,,

* On a *newly* ecdysed crab there was no trace of lime-salt deposition, but in THIS example carbonate of lime had been deposited to some extent before the death of the animal.

TABLE VI. *Showing Comparative Amount of Calcareous and Organic Matter in Edible Crab* (Anderson).

	Percentage Composition.
Water, blood, salts, &c., ...	66·46
Flesh (gave 14·56 of ash, containing 4·94–$3CaO,PO_5$)	2·95
Outer calcareous structure, ...	29·56
Inner calcareous structure, ...	1·03
	100·00

TABLE VII. (p. 86). *Showing Composition of Precipitate thrown out of Mixture of Sea Water and Urine after standing Seven Days at* 70° F. (Anderson).

Water and organic matter containing ammonia (7·38 per cent.), .	31·81
Carbonate of lime, ...	4·85
Phosphate of magnesia and ammonia, ...	51·10
Phosphate of lime, ...	12·24
	100·00

Table VIII. (p. 87). *Showing Composition of Precipitate thrown out of Mixture of Sea Water and Urine (after filtration from Precipitate which was thrown out in Seven Days), standing other Ten Days at* 70° F. (Anderson).

Water and organic matter,	20·25
Carbonate of lime,	75·35
Carbonate of magnesia,	1·02
Phosphate of magnesia,	3·38
	100·00

Table IX. *Showing Saline and Albuminoid Ammonia (or Nitrogen) in* 1,000,000 *parts of Sea Water* (Anderson).

Water taken from immediate vicinity of coral islands (Louisiade Group) contains—	
Saline ammonia,	0·48
Albuminoid nitrogen, taken as ammonia,	0·18
	0·66
Water from North Atlantic (lat. 30° 20′ N., long. 36° 6′ W.)—	
Saline ammonia,	0·26
Albuminoid nitrogen,	0·16
	0·42
Water from German Ocean, near land—	
Saline ammonia,	0·13
Albuminoid nitrogen,	0·13
	0·26

Table X. *Showing Solubilities of Various Forms of Coral, &c., in Sea Water.*

Analysts—Young and Anderson.	Temperature °C.	Exposure, in hours.	Amount Soluble.	
			In grammes $CaCO_3$ per litre.	In parts of Sea Water. One part in
Coral sand,	27	12	0·0320	32,000
Harbour mud, Bermuda,	27	12	0·0410	25,000
Isophyllia dipsacea (Dana), Bermuda,	27	12	0·0410	25,000
Millepora ramosa (Pallas), Bermuda,	27	12	0·0360	28,500
Madrepora aspera (Dana), Mactan Isld., Zebu,	27	12	0·0730	14,000
Montipora foliosa (Pallas), Amboina,	27	12	0·0430	23,800
Goniastræa multilobata (Quelch), Amboina,	10	12	0·0730	14,000
Porites clavaria (Lamk.), Bermuda,	11	12	0·0930	11,000
Melobesia, Kilbrennan Sound,	10	12	0·0890	11,500
Oculina coronalis (powdered finely),	10	96	0·0237	42,600

(These were in summer water from German Ocean.)

TABLE XI. *Showing Solubilities of various Molluscs and Crustacea.*

Analysts—Young and Anderson.	Temperature ° C.	Exposure, in hours.	Amount Soluble.	
			In grammes $CaCO_3$ per litre.	In parts of Sea Water. One part in
Mussels allowed to rot in water for 7 days, .	17	168	0·3840	2,617
Lobsters ,, ,, 3 weeks, .	10	504	1·0620	966
Shrimps ,, ,, 3 ,, .	10	504	1·0470	980
Schizopoda ,, ,, 3 ,, .	10	504	0·7820	1,309

TABLE XII. *Showing Action of Winter Waters on Carbonate of Lime.*

Analyst—Anderson.	Temperature ° C.	Exposure, in hours.	Amount Soluble.	
			In grammes $CaCO_3$ per litre.	In parts of Sea Water. One part in
A winter water from the Baltic Sea, with density only 1005·65, dissolved of powdered *Oculina coronalis*, ...	10	18	0·0110	93,300
A winter water from North Berwick shore did not dissolve any of the crystalline or coral forms of carbonate of lime. Another winter water from the North Sea did not dissolve any, nor did the water taken at Granton shore. But when a quantity of carbonic acid water was added to the North Berwick water (insufficient to destroy its alkaline character), it dissolved increasing quantities of coral, &c., as—				
Powdered *Oculina coronalis*, ...	10	18	0·0670	15,300
Globigerina ooze (whole), ...	10	18	0·0075	136,800
,, (powdered), ...	10	192	0·0340	30,200

TABLE XIII. *Showing the Solubilities of Crystalline Forms of Carbonate of Lime in Sea Water.*

Analysts—Anderson and Drysdale.	Temperature ° C.	Exposure, in hours.	Amount Soluble. In grammes $CaCO_3$ per litre.	Amount Soluble. In parts of Sea Water. One part in
Calcspar, massive,	10	47	0·0075	136,800
,, ,,	10	120	0·0046	223,000
,, ,,	10	396	0·0000	...
,, finely powdered,	10	47	0·0082	125,000
,, ,, ,,	10	120	0·0052	197,000
,, ,, ,,	10	396	0·0000	...

TABLE XIV. *Showing Solubility in Pure Distilled Water.*

Analyst—Anderson.	Temperature ° C.	Exposure, in hours.	Amount Soluble. In grammes $CaCO_3$ per litre.	Amount Soluble. In parts of Sea Water. One part in
Calcspar, massive,	10	120	0·0146	68,500
,, impalpable powder,	10	46	0·0251	39,800
Oculina coronalis (powdered),	10	96	0·0285	35,000
Amorphous $CaCO_3$ (greater part crystallised out again immediately),	10	...	0·2480	4,032

TABLE XV. *Showing Solubility in Pure Carbonic Acid Water at Atmospheric Pressure.*

Analyst—Drysdale.	Temperature ° C.	Exposure, in hours.	Amount Soluble. In grammes $CaCO_3$ per litre.	Amount Soluble. In parts of Sea Water. One part in
Calcspar, massive,	10	24	0·0815	12,270
,, granular,	10	24	0·1285	7,780
,, fine-grained,	10	24	0·2036	4,910
,, impalpable powder,	10	24	0·4720	2,120

TABLE XVI. *Showing Influence of Pressure on the Solubility of Carbonate of Lime in Sea Water containing Carbonic Acid* (W. G. Reid).

No. of Experiment.	Capacity of Funnel.	Carbonate of Lime used.		Quantity of Sea Water taken.	Carbon Dioxide. Taken.		Carbon Dioxide. Per Litre Sea Water.		Pressure.		Temperature.	Resulting Alkalinity per Litre.	Res. Alk. (m), minus original Alkalinity.	(n) = $CaCO_3$ Dissolved per Litre Sea Water.	$CaCo_3$ Dissolved per grm. CO_2 (i) taken.
		Kind.	Quantity.		Vol.	Weight.	Vol.	Weight.	Amount.	Time at Maximum.					
(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)	(j)	(k)	(l)	(m)	(n)	(o)	(p)
	c.c.		grms.	c.c.	c.c.	grms.	c.c.	grms.		minutes.	°			grms.	grms.
I.	65·5	Glob. I.	1·5092	106·65	54·33	0·1074	509·4	1·0071	4 tons.	30 to 60	12·0	91·5	39·9	0·0906	0·0904
II.	65·5	Do.	1·5867	117·53	62·39	0·1233	530·8	1·0494	4 ,,	Do.	12·0	117·0	65·4	0·1486	0·1416
III.	84·4	Do.	1·5697	117·53	62·56	0·1237	532·3	1·0523	4 ,,	Do.	9·0	104·0	52·4	0·1191	0·1106
IV.	65·5	Do.	1·5800	106·65	55·04	0·1088	516·1	1·0203	4 ,,	Do.	9·0	99·1	47·5	0·1080	0·1060
V.	65·5	Do.	1·5020	106·65	54·50	0·1077	511·0	1·0102	Atmospheric.	Do.	12·2	76·6	25·0	0·0568	0·0562
VI.	84·4	Do.	1·5620	117·54	63·84	0·1263	543·4	1·0744	Do.	Do.	10·0	76·8	25·2	0·0572	0·0546
VII.	91·1	Do.	1·5620	117·53	64·06	0·1266	545·0	1·0777	2 tons.	Do.	8·3	95·6	44·0	0·0999	0·0927
VIII.	65·5	Glob. II.	1·5030	106·65	55·43	0·1096	519·7	1·0275	4 ,,	Do.	8·6	85·1	33·5	0·0761	0·0758
IX.	84·4	Do.	1·5610	117·54	63·42	0·1254	539·6	1·0668	4 ,,	Do.	8·8	95·5	43·9	0·0997	0·0935
X.	65·5	Do.	1·5020	106·60	53·92	0·1066	505·9	1·0001	Atmospheric.	Do.	15·0	61·9	10·4	0·0236	0·0236
XI.	84·4	Do.	1·5600	117·50	59·67	0·1180	507·8	1·0039	Do.	Do.	13·2	63·4	11·9	0 0270	0·0269
XII.	91·1	Coral sand I.	1·5580	117·54	63·40	0·1256	539·4	1·0664	4 tons.	Do.	8·8	113·1	61·5	0·1398	0·1311
XIII.	65·5	Do.	1·5025	106·65	54·57	0·1079	511·4	1·0115	4 ,,	Do.	8·8	96·0	44·4	0·1011	0·0999
XIV.	91·1	Do.	1·5370	117·50	61·82	0·1222	526·2	1·0402	Atmospheric.	Do.	11·6	71·4	18·9	0·0429	0·0413
XV.	65·5	Do.	1·5020	106·60	54·29	0·1073	509·3	1·0069	Do.	Do.	10·5	70·9	18·4	0·0418	0·0425
XVI.	91·1	Do.	1·5380	117·50	57·45	0·1135	488·9	0·9666	Do.	Do.	12·2	69·42	17·92	0·0407	0·0421
XVII.	91·1	Coral sand II.	1·5595	117·54	63·04	0·1246	536·0	1·0596	4 tons.	Do.	9·1	105·3	53·7	0·1220	0·1152
XVIII.	91·1	Do.	1·5600	117·53	63·81	0·1261	542·9	1·0738	Atmospheric.	Do.	9·0	68·8	17·2	0·0391	0·0364
XIX.	91·1	Pteropods.	0·9850	117·54	64·08	0·1267	545·2	1·0779	4 tons.	Do.	8·3	99·9	48·3	0·1098	0·1019
XX.	91·1	Crystal.	3·0027	117·52	62·76	0·1244	533·9	1·0557	Atmospheric.	Do.	11·0	74·3	22·7	0·0516	0·0490
XXI.	91·1	Do.	...	117·52	61·27	0·1211	521·4	1·0307	4 tons.	Do.	8·3	67·9	16·3	0·0371	0·0360
XXII.	91·1	Do.	...	106·65	53·22	0·1052	499·0	0·9865	4 ,,	Do.	9·0	57·4	5·8	0·0132	0·0137
XXIII.	91·1	Do.	...	106·65	53·12	0·1050	498·0	0·9845	Atmospheric.	Do.	10·0	56·1	4·5	0·0102	0·0104
XXIV.	91·1	Do.	3·1387	117·48	61·85	0·1223	526·5	1·0410	Do.	Do.	12·2	55·1	3·6	0·0082	0·0078
XXV.	91·1	Do.	3·1379	117·53	62·50	0·1235	531·7	1·0513	Do.	Do.	12·2	53·7	2·2	0·0050	0·0047
XXVI.	91·1	Do.	3·1371	117.49	62·04	0·1226	528·0	1·0440	Do.	Do.	13·1	53·0	1·5	0·0034	0·0032
XXVII.	91·1	Do.	3·1355	117·55	62·62	0·1238	532·7	1·0532	Do.	Do.	14·7	53·6	2·1	0·0048	0·0045
XXVIII.	91·1	Do. ground.	2·9938	117·50	62·62	0·1238	532·9	1·0536	Do.	Do.	12·6	65·7	14·2	0 0322	0·0306

TABLE XVII. *Showing the Effect of Simple Immersion in Sea Water on Certain Corals.*

The following series of experiments as to the solubility of Coral simply suspended in mass in sea water and allowed to remain under, as nearly as possible, the same conditions, for certain periods of time, was made by Mr J. Galbraith Ross, B.Sc.

The corals were dried at 100°–110° C., until the weight remained constant.

The temperature of the water ranged from a little over freezing-point to 40° F.

No.	Species.	Locality.	Weight in grammes.	Surface in square inches.	Time of Immersion in days.	Loss.
1.	*Oculina varicosa,*	St Thomas, W. I.	16·3164	8	20	0·0748
2.	*Madrepora scabrosa,*	Levuka, Fiji.	21·8540	16	30	0·1497
3.	*Montipora foliosa,*	Amboyna.	15·3334	15	46	0·1223
4.	*Goniastræa multilobata,*	,,	26·4069	12	60	0·0895
5.	*Millepora ramosa,*	Bermuda.	14·2373	5	60	0·1513
6.	*Millepora murrayi,*	Samboangan.	13·0605	8	60	0·0725
7.	*Fungia discus,*	...	44·3132	acciden	tally de	stroyed.
8.	*Mæandrina strigosa,*	Bermuda.	22·9932	10	45	0·1447
9.	*Madrepora aspera,*	Mactan, Zebu.	25·3694	10	45	0·0734
10.	*Madrepora palmata,*	St Thomas, W. I.	31·7357	10	45	0·1707

Reducing these results to unit surface and time, the relation between them will be better observed:—

No.	1.	Loss by solution from 1 square inch	per annum,		0·1706.
,,	2.	,,	,,	,,	0·1138.
,,	3.	,,	,,	,,	0·0647.
,,	4.	,,	,,	,,	0·0453.
,,	5.	,,	,,	,,	0·1860.
,,	6.	,,	,,	,,	0·0552.
,,	8.	,,	,,	,,	0·1173.
,,	9.	,,	,,	,,	0·0584.
,,	10.	,,	,,	,,	0·1384.

The above series proves that under the same conditions different species of coral are acted upon in different degrees by the solvent action of sea water. Further, it would appear that the more areolar or amorphous the coral is the greater the action as compared with the harder crystalline varieties.

Note on Ripples in a Viscous Liquid. By Prof. Tait.

(Read March 3, 1890.)

The following investigation was made in consequence of certain peculiarities in the earlier results of some recent measurements of ripples by Prof. Michie Smith, in my Laboratory, which will, I hope, soon be communicated to the Society. These seemed to suggest that viscosity might have some influence on the results, as might also the film of oxide, &c., which soon gathers on a free surface of mercury. I therefore took account of the density, as well as of possible rigidity, of this surface layer, in addition to the surface tension which was the object of Prof. Smith's work. The later part of the paper, where Cartesian coördinates are employed, runs somewhat on the lines of an analogous investigation in Basset's *Hydrodynamics*. My original object, however, was different from his, as I sought the effects of viscosity on waves steadily maintained by means of a tuning-fork used as a current interruptor; not on waves once started and then left to themselves. Besides obtaining his boundary conditions in a singular manner, I think that in his § 521 Mr Basset has made an erroneous investigation of the effects of very great viscosity.

The stress-function in a viscous liquid may be obtained (*Phil. Mag.*, Jan. 1890) from that in an elastic solid, by substituting velocity for displacement; in the form

$$\phi\omega = -\mu(S\omega\nabla.\sigma + \nabla S\omega\sigma) - (c - \tfrac{2}{3}\mu)\omega S\nabla\sigma \quad . \quad . \quad (1)$$

where, in order to include the part of the pressure which is not due to motion, we must write p instead of the quantity

$$cS\nabla\sigma .$$

Here σ is the vector velocity of an element at ρ, and μ is the coefficient of viscosity.

Hence, supposing the volume of the element to be unity, we have for the equation of motion

$$e\frac{\partial\sigma}{\partial t} = -\nabla(eP) + \iint\phi U\nu ds$$
$$= -\nabla(p + eP) - \mu(\nabla^2\sigma + \tfrac{1}{3}\nabla S\nabla\sigma),$$

where e is the density of the liquid, and P the potential energy of unit mass at ρ; and the double integral is taken over the surface of the element. This is a perfectly general equation, so we must proceed to the necessary limitations.

First. Let the displacements be so small that their squares may be neglected. Then we may write d for ∂.

Second. Let the liquid be incompressible; then

$$S\nabla\sigma = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

With these, the equation of motion becomes

$$e\frac{d\sigma}{dt} = -\nabla(p + eP) - \mu\nabla^2\sigma \quad . \quad . \quad . \quad . \quad . \quad (3).$$

Third. Let the motion be parallel to one plane, and we have

$$Sk\sigma = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

From (2) and (4) we have at once

$$\sigma = \nabla w.k \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where w is a scalar function of $Vk\rho$.

Operate on (3) by $V.\nabla$, and substitute from (5), and we have

$$\left(e\frac{d}{dt} + \mu\nabla^2\right)\nabla^2 w = 0 \quad . \quad . \quad . \quad . \quad . \quad (6).$$

Fourth. Limit w to disturbances which diminish rapidly with depth. Here the problem has so far lost its generality that it is advisable to employ Cartesian coordinates, the axis of x (i) being in the direction of wave motion, and that of y (j) vertically upwards. Then it is clear that a particular integral of (6) is

$$w = (A\varepsilon^{ry} + B\varepsilon^{sy})\varepsilon^{(rx+nt)\iota} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where ι denotes $\sqrt{-1}$. The only conditions imposed on r, s, and n, are that the real parts of r and s, in so far as they multiply y, must be positive; and by (6)

$$\mu(s^2 - r^2) = en\iota \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

The speed of vertical displacement of the surface is found from

$$\eta = -(Sj\sigma)_0 = (Si\nabla w)_0 = -r\iota(A + B)\varepsilon^{(rx+nt)\iota}, \quad . \quad . \quad (9).$$

From this, $\dfrac{d^2\eta}{dx^2}$ and $\dfrac{d^4\eta}{dx^4}$, which will be required below, are found by using the factors $-r^2$ and r^4.

The stress on the free surface (where $y=\eta$, a quantity of the order A) is, by (1),

$$(\phi j)_0 = -p_0 j - \mu(\mathrm{S}j\nabla.\sigma + \nabla\mathrm{S}j\sigma)_0 \quad . \quad . \quad (10)$$

where, in p_0, we must include the effects of the tension T, and of the flexural-rigidity E, of the surface-film.

But, by (3) and (5), we have

$$\nabla(p+e\mathrm{P}) = -(e\frac{d}{dt} + \mu\nabla^2)\nabla w.k\,;$$

so that as

$$\mathrm{P}=gy$$

we have

$$dp + egdy = en\iota(r\iota dy - rdx)\mathrm{A}\epsilon^{ry+(rx+nt)\iota}\,.$$

From this, by integrating, and introducing the surface conditions,

$$\Pi - p_0 = eg\eta - \mathrm{T}\frac{d^2\eta}{dx^2} + \mathrm{E}\frac{d^4\eta}{dx^4} + en\mathrm{A}\epsilon^{(rx+nt)\iota},$$

If we now substitute this in (10) and, for the boundary condition,* make

$$\frac{d}{dt}(\phi j)_0 = 0\,,$$

(omitting terms of the second degree in A and B), we have by means of (9) the two equations

$$\mathrm{R}(\mathrm{A}+\mathrm{B}) - en^2\mathrm{A} + 2\mu rn\iota(r\mathrm{A}+s\mathrm{B}) = 0\,,$$

$$r^2(\mathrm{A}+\mathrm{B}) + r^2\mathrm{A} + s^2\mathrm{B} = 0\,,$$

where, for shortness,

$$\mathrm{R} = egr + \mathrm{T}r^3 + \mathrm{E}r^5 \quad . \quad . \quad . \quad . \quad . \quad . \quad (11).$$

Thus, finally,

$$\mathrm{R} - en^2 + 4\mu nr^2\iota + 4\frac{\mu^2}{e}r^3(r-s) = 0 \quad . \quad . \quad (12).$$

This must be treated differently according as μ is small or great.

I. Let μ be small; and let n be given, and real. This is the case of the sustained waves in Prof. Smith's experiments.

The equation obtained by neglecting μ, viz.

$$\mathrm{R} - en^2 = 0$$

* W. Thomson, *Camb. and Dublin Math. Journal*, iii. 89 (1848).

gives one, and only one, positive value of r, whose value is diminished (*i.e.*, the wave-length is increased) alike by surface tension and by surface-flexural-rigidity. Call it r_0, and let

$$r = r_0 + \mu\rho\,,$$

then by (12), keeping only terms of the first order in μ,

$$(ge + 3\mathrm{T}r_0^2 + 5\mathrm{E}r_0^4)\rho + 4nr_0^2\iota = 0 \quad . \quad . \quad . \quad . \quad . \quad (13).$$

Thus ρ is a pure imaginary, and therefore the viscosity does not affect the length of the waves. It makes their amplitude diminish as they leave the source. (For the real part of w belongs in this case, if we take n as positive, to waves travelling in the negative direction along x, and *vice versâ.*) The factor for diminution of amplitude per unit distance travelled by the wave is

$$\epsilon^{-\mu\rho\iota}.$$

This expression gives very curious information as to the relative effects of viscosity on the amplitudes of long and short waves, when we suppose gravity, surface-tension, or surface-flexural-rigidity, *alone*, to be the cause of the propagation.

If the waves be started once for all, and allowed to die out, r is given and n is to be found. This is the first case treated by Mr Basset. If then $n = n_0$ be found from

$$en^2 = \mathrm{R}\,,$$

we may put

$$n = n_0 + \mu\nu\,.$$

By (12) we have, keeping only the first power of μ,

$$e\nu = 2r^2\iota\,,$$

which coincides with the result given in § 520 of Basset's *Treatise.*

II. Let μ be large. Suppose r to be given, a real positive quantity. Then, by (8), we may eliminate n from (12) and obtain

$$\frac{\mathrm{R}e}{\mu^2} + (s^2 - r^2)^2 + 4r^2(s^2 - r^2) + 4r^3(r - s) = 0 \quad . \quad . \quad . \quad (14).$$

The first term is very small, and the rest has the factor $s - r$. Omit the term which contains this factor twice, and we have

$$s(s - r) = -\frac{\mathrm{R}e}{4r^2\mu^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (15).$$

This has real positive roots if, and only if,

$$\mu^2 r^4 > Re\,,$$

and thus, by (8), when this condition is satisfied n is a pure imaginary, and there can be no oscillation. Of the two roots of (15) we must, in consequence of our assumption (that $(s-r)^2$ is negligible) choose that which is nearly equal to r. It might be fancied that, as this assumption leads to $B = -A$ very nearly, a new limitation would be introduced as regards the magnitude of η. But we have

$$\eta = -\frac{r}{n}(A+B)\epsilon^{(rx+nt)\iota} = -\frac{r}{n}A\left(1-\frac{2r^2}{r^2+s^2}\right)\epsilon^{(rx+nt)\iota}$$

$$= -A\frac{e\iota}{\mu r}\epsilon^{(rx+nt)\iota}, \text{ nearly.}$$

The wave-pattern, in this case, does not travel but subsides *in situ*, its amplitude diminishing according to the approximate factor

$$\epsilon^{-Rt/2\mu r^3}.$$

Thus, as was to be expected, the subsidence is slower as the friction is greater. Also, if gravitation is the sole cause of subsidence, the longer waves subside the faster; while if the main cause be surface-tension, or surface-flexural-rigidity, the shorter waves subside the faster.

III. If there be a uniform film of oxide or dust, in *separate* particles which adhere to and move with the surface, we must add to the expression for surface-stress in (10) the term

$$-m(\dot{\sigma})_0 = -m\frac{d}{dt}(\nabla w)_0 k$$

$$= -m\big(jrn(A+B) + in(rA+sB)\big)\epsilon^{(rx+nt)\iota}\,,$$

where m is the surface-density of the film.

The equations for the elimination of A/B become

$$(R + mrn^2 - en^2 + 2\mu r^2 n\iota)A + (R + mrn^2 + 2\mu rsn\iota)B = 0\,,$$

$$\left(2r^2 - \frac{mnr\iota}{\mu}\right)A + \left(2r^2 + \frac{en\iota}{\mu} - \frac{mns\iota}{\mu}\right)B = 0\,;$$

so that instead of (12) we have

$$\big(e - m(s-r)\big)(R + mrn^2) = e^2n^2 - 4\mu r^2 ne\iota + 4\mu^2 r^3(s-r) - mn^2 es\,.$$

When μ and m are small, this is approximately

$$R + 2mrn^2 = en^2 - 4\mu r^2 n\iota .$$

There is no other term in the first power of m, independent of μ; so that, to this degree of approximation (which is probably always sufficient), the dust layer has no effect except to increase R. When there is no viscosity this increases the ripple-length (*i.e.*, diminishes r) for a given period of vibration.

When terms of the first degree in the viscosity are taken account of, the effect on n (for a given value of r) is merely to add to it the pure imaginary

$$2\mu r^2\iota/(e - 2mr),$$

whose value increases alike with m and with r.

Thus the period is not affected, but the surface layer aids viscosity in causing waves to subside as they advance.

This investigation above may be easily extended to the case in which a thin liquid layer is poured on mercury to keep its surface untarnished. The only difficulty is with respect to the relative tangential motion at the common surface of the liquids.

The Determination of Surface-Tension by the Measurement of Ripples. By C. Michie Smith.

(Read March 17, 1890.)

Professor Tait has shown* that accurate determinations of surface-tensions would be obtained if we could measure the rate of propagation of ripples set up by the vibration of a tuning-fork of known pitch. The relation between the rate of propagation (v) of the ripples and the surface-tension (T) is given by the formula

$$v^2 = \frac{g\lambda}{2\pi} + \frac{2\pi}{\lambda} \cdot \frac{T}{\rho},$$

where λ is the ripple-length and ρ the density of the liquid. If for v we write λ/t, where t is the vibration period of the fork, the equation can be written

$$T = \frac{\lambda^3\rho}{2\pi t^2} - \frac{g\lambda^2\rho}{4\pi^2}.$$

* *Proc. Roy. Soc. Edin.*, 1875, p. 485.

The second term on the right-hand side of the equation is the part depending on gravity, and it is very small relatively to the other term. In the case of mercury, taking the greatest wave-length employed, its value is only 1/2040 of the first term, a quantity far within the limits of observational error. I have accordingly used in all the calculations the form $T = \lambda^3\rho/2\pi t^2$. If in this λ be measured in centimetres and t in seconds, T is given in dynes per centimetre. Previous attempts made by myself and others to determine surface-tensions by this method failed on account of the difficulty experienced in measuring the lengths of the ripples, and since $T \propto \lambda^3$, it is evident that the method is useless unless λ can be measured with great accuracy. This difficulty has been overcome by the use of photography, and it is now possible to test the practical value of the method by comparing the results that have been obtained with those got by other means. The application of photography is easy, since the waves that are dealt with are "standing" waves, and when the apparatus is in proper adjustment the photograph is sharp even with exposures of as much as 10 seconds. The usual exposure is from 1 to 2 seconds. Thus there is an essential difference between this application of photography and that made by Lord Rayleigh, who photographs a moving stream in a small fraction of a second.

The general plan of procedure is as follows:—A tuning-fork in which the vibrations are electrically maintained is employed to produce ripples on the surface of the liquid under experiment, which is contained in a shallow dish. A scale is suitably placed on the edge of the dish so that its divisions are as nearly as possible parallel to the crests of the ripples, and a photograph of the dish is then taken. This photograph is measured with a micrometer, and the measurements are interpreted by reference to those of the scale photographed on the same plate.

In the earlier experiments much difficulty was experienced in getting suitable illumination, but ultimately it was found that the best photographs were got simply by placing the dish near a window and using the light from the sky or a bright cloud. In most cases it was found best to allow a shadow of a bar of the window or of the tuning-fork itself to fall on the surface, for the edge of such a shadow becomes regularly serrated, and this serration is easily

photographed. The distance between the ripple-crests is easily measured in such a photograph, but there is some risk of not measuring exactly at right angles to the lines of crests.

The method of exciting the ripples was found to be of great importance. In the first experiments the driving-fork of a Helmholtz vowel-sound apparatus was taken, and the dipper attached to one of the prongs was replaced by a light arm of aluminium, bent downwards at right angles near the end. The point of this was smoothed and tapered, and was allowed to dip slightly into the liquid. When the fork vibrated, the point set up a series of circular ripples which were fairly steady and uniform. A number of photographs were taken with this arrangement, but on measuring them it was found that they gave for mercury a wave-length of only 0·104 cm. for 256 vibrations per second, corresponding with $T = 159$. The value for T given by Quincke is 540, and the difference was evidently far too great to be accounted for by errors of observation. During these experiments it was noticed that very perfect ripples were produced when a vessel containing mercury was laid on the sounding-box of a tuning-fork, and a large series of measurements were made in this way. Using the same fork of 256 vibrations per second the photographs gave $\lambda = 0·108$ cm., which differed but little from the previous results. Experiments were then made to determine whether the shape, size, or material of the dish had any influence on the length of the ripples, but the slight variations observed in the value of λ were only such as might easily be explained by differences in the temperature or in the cleanness of the surface. The tuning-fork was then changed for one giving 512 vibrations per second, but this, too, gave approximately the same value for T. Finally, a fork giving 768 vibrations per second was used, and it was found that with a round glass vessel a result was obtained which differed widely from previous results. On substituting a rectangular ebonite dish for the glass one it was found to be impossible to get ripples distinct enough to be photographed by simply laying the dish on the sounding-board, and consequently experiments were tried with the edge of the dish pressing against one prong of the tuning-fork low down. This proved most effective, and the first photograph gave $\lambda = 0·075$ corresponding to $T = 528$ —a result differing only about 2 per cent. from Quincke's mean value.

This plan of making the dish actually touch the fork was adopted in all the subsequent experiments, and yielded consistent results.

The cause of the failure of the earlier methods is not perfectly clear. The average value of the wave-length in mercury for 256 vibrations per second was found from the ten best photographs to be 0·108 cm., the values varying from 0·102 to 0·112 cm. If we take Quincke's value for T, viz., 540 dynes per centimetre, this would correspond with a note of 445 vibrations per second.

In the cases in which a dipper was used the explanation may be found in the circumstance that two impulses are given to the mercury for each complete vibration of the fork, one as the dipper enters, the other as it leaves, the liquid. If this is the true source of error it might be supposed that there is a somewhat similar cause acting when the vessel rests on the sounding-box, for in that case too it must receive two impulses for each vibration. On the other hand, when the dish presses lightly on the prong of the fork it would receive only one impulse for each vibration. But if the above explanation were complete one would expect to get ripples due to a note an octave above that of the fork, whereas those actually got correspond almost exactly to A♯ of the same octave, the fork giving the C below. I propose to examine this point more carefully hereafter.

As finally arranged, the driving-fork was placed at a distance and was connected by loose wires with the fork—one of the series belonging to a Helmholtz vowel-sound apparatus—actually used to set up the ripples. This was placed on a stone slab in front of a window so that it could be illuminated by the light from the sky. The vessel usually employed was an ebonite developing dish, either of half-plate or quarter-plate size. This dish was supported near the fork in such a way as to touch one of the prongs at a point as low down as possible. The mercury or other liquid was then poured in till a depth was reached, beyond which any increase seriously diminished the distinctness of the ripples. If the contact with the dish be made at a point taken at random a very complex and unsteady series of ripples will usually be produced, but with a little care it is possible to find a point which yields two steady series of ripples crossing each other at right angles. Except with the eye nearly on a level with the surface of the liquid, the ripples can be

seen distinctly only in one particular direction, and care must be taken to place the axis of the camera in that direction. If the camera could be placed vertically over the dish it would greatly simplify the measurement of the plates, since the scale-value would be the same in all directions. This, however, is seldom practicable, and usually the axis of the camera has to be inclined to the vertical at a considerable angle, so that the scale-value varies not only for different parts of the plate, but also for different directions. The corrections to be applied cannot easily be calculated, but they can be got by actual measurement. For this purpose a sheet of paper was taken of the size of the dish, and on it a number of circles were described with a radius of 1 cm. This was then laid on the surface of the mercury and photographed. The scale value for any part of the mercury surface in any direction can be obtained by measuring the diameter of the corresponding circle in the same direction.

The results so far obtained can only be looked on as preliminary, but they are sufficient to test the value of the method. At present the value of the wave-lengths is probably not trustworthy to within less than 2 per cent., but there seems no reason to doubt that this uncertainty can be reduced to at least one-half by the introduction of certain modifications in the apparatus. An error of 2 per cent. in the wave-length corresponds to an error of approximately 6 per cent. in the value of T, and the extreme values obtained differ from each other by somewhat less than this in the case of mercury. Quincke's results for mercury* which are usually accepted, vary from 511 to 572 dynes per centimetre, with a mean value of 540; so that his extreme values differ from each other by about 12 per cent.

In his paper read at last meeting of the Society, Professor Tait showed that if we take account of the surface-flexural rigidity (E), the equation for the wave-length takes a form which can be written :—

$$T = \frac{\lambda^3 \rho}{2\pi t^2} - \frac{g\rho\lambda^2}{4\pi^2} - \frac{4\pi^2 E}{\lambda^2}.$$

With small values of λ the factor $\frac{4\pi^2}{\lambda^2}$ becomes very large, and unless E is excessively small it ought to be possible to detect its influence by comparing the results got for two forks of very different

* *Pogg. Annal.*, cv. p. 1.

pitch. The results obtained so far are not sufficient to enable any accurate comparison to be made, but they show that if there is any effect produced by the surface-flexure rigidity it must be very small compared with that due to surface-tension.

The following table gives the actual results for mercury at a mean temperature of about 12° C.

Vibrations per Second.	λ Centimetres.	T Dynes per Centimetre.
768	0·0745	528
,,	0·0746	531
	Average	529·5
512	0·0980	534
,,	0·0971	519
	Average	526·5
256	0·157	546
,,	0·154	517
	Average	531·5

Average for the whole, 529.

Two photographs were taken for water, but in each of them only a few waves could be measured, so that no great weight can be placed on the result. They were both with the fork giving 256 vibrations per second, and the resulting wave-lengths were 0·195 and 0·208 cm., corresponding to the values of 77 and 94 for T. The mean is 85·5, while Quincke's value is 81.

A photograph of a mercury surface covered with dilute sulphuric acid gave $\lambda = 0{\cdot}141$ for 256 vibrations per second. If we neglect all influence of the acid other than in changing the surface-tension, this would give $T = 400$, or practically the same as for water and mercury.

Two photographs were taken of a surface of mercury charged to a high potential by means of a Holtz electric machine. The photographs gave very consistent measures :—

No. 43. 256 v.p.s. Average of 58 waves $\lambda = 0{\cdot}143$ . . $T = 417$.

No. 44. 256 v.p.s. Average of 30 waves $\lambda = 0{\cdot}144$ . . $T = 425$.

So that it is evident that electrification produces a considerable

reduction in the surface-tension, though the action may be a purely mechanical one.

I hope to continue and extend the experiments at an early date; meanwhile my thanks are due to Professor Tait for many valuable suggestions, and to Mr D. J. Graham who has assisted me in all the experiments.

The Absorption Spectra of Certain Vegetable Colouring Matters. By C. Michie Smith. (With a Plate.)

(Read March 17, 1890.)

For the substances dealt with in this paper I have been indebted at various times, to the kindness of Mr David Hooper, Quinologist to the Madras Government. The observations have been made with several instruments, but have all been reduced to the scale of the direct vision spectroscope of the Chemical Laboratory of the Edinburgh University, kindly placed at my disposal for the most important part of the work. The illumination used with this instrument is a 10-candle power incandescent lamp; with the other instruments sun-light was used. I have thought it better for practical purposes to draw the spectra to the natural scale of the instrument than to reduce them to a scale of wave-lengths, but data are supplied for finding the wave-length corresponding to any part of the spectrum. To represent the varying shades of an absorption spectrum is extremely difficult, and the results usually obtained are, at best, far from satisfactory. The method I have employed is to draw the spectra on a large scale, shading the various parts by aid of a light curve, and then reduce this drawing to the required size by photography. The result, though not all that could be desired, is fairly satisfactory.

Colouring Matter from Trichosanthes palmata.—Mr Hooper has given me the following notes on this substance:—"The plant which yields it is the *Trichosanthes palmata*, one of the Gourd tribe or Cucurbitaceæ, and the rounded scarlet fruits have their seeds imbedded in a green bitter pulp. The bitter principle is a glucoside, soluble in water and alcohol, and affords a red solution, changing to purple with sulphuric acid. I have named it 'trichosanthin,' as

it differs from colocynthin. The green matter is best prepared by agitating the dry pulp with ether, evaporating down and dissolving the ethereal residue in strong spirit. In this manner the bitter principle and fatty matters are eliminated." The solution closely resembles a solution of chlorophyll of equal strength. Both are beautifully dichroic, appearing green in thin, and red in thicker layers; both also have a red fluorescence. The chief difference on inspection is that a much thicker layer of chlorophyll is required to yield the red colour by transmitted light.

Spectrum.—The spectroscope at once shows that this green colouring matter is not chlorophyll. Spectra (*a*), (*b*), and (*c*), in the plate are for different thicknesses of the trichosanthes colouring matter, while (*d*) gives the spectrum of chlorophyll from cabbage of a thickness and strength corresponding as nearly as possible with (*b*). Taking this as the most characteristic spectrum, it may be thus described:—The spectrum begins at 1736, and slight shading continues to 1693. The first band begins (penumbra) at 1638 (W. L. 654·1), and ends about 1600 (W. L. 615), the maximum absorption being from 1631 to 1627, and from 1605 to 1602. From this there is a small amount of absorption till the second band begins at 1576 (W. L. 593·4), and continues to 1543 (W. L. 566·8), the maximum absorption being from 1570 to 1563. From this there is no perceptible absorption till the third band, which begins at 1516 (W. L. 548·4), and continues to 1495 (W. L. 534·8), the maximum darkness being from 1511 to 1501. There is a fourth band, very faint, with its centre about 1452 (W. L. 510·6), and a fifth extending from 1400 (W. L. 485) to 1371. The spectrum ends about 1355 (W. L. 467). The relative depths of the maxima of these five bands are 10, 5, 4, 1½, 1. When a thin layer of the solution is used the spectrum shows two strongly marked bands from 1625 to 1612 (darkness 10), and from 1569 to 1550 (darkness 4), and a slight trace of a third band with its centre at 1501. The spectrum ends sharply at 1340. With a very thick layer the first band widens out in both directions, and strong absorption—sufficient to mask bands II. and III.—continues to 1485. There is also much increased absorption in the blue. How completely these spectra differ from that of chlorophyll will best be seen from the plate. The chief band of chlorophyll lies

between 1665 (W. L. 789) and 1625 (W. L. 639·8), while the chief band in a thin film of trichosanthes would almost exactly fill up the interval between bands I. and II. in chlorophyll. Bands III., IV., and V. of trichosanthes may be coincident with faint chlorophyll bands.

The positions of some of the chlorophyll bands, as is well known, can be changed by the action of reagents, and the trichosanthes bands are even more strongly affected. The first reagent experimented with was sulphide of ammonia, which probably has a reducing action. The result is shown in spectra (*e*), (*f*) and (*g*), which represent the appearance after 2, 11, and 39 days respectively, after which there was no further change. Figure (*h*) gives the spectrum of chlorophyll similarly treated. The change in the trichosanthes spectrum is complete. Band I. gradually becomes weaker, and finally vanishes; two new bands appear in the space between bands I. and II. of the original spectrum; band II. is apparently displaced towards the violet end and intensified; and band IV. is greatly widened. Chlorophyll is much less affected, and the difference between two spectra could hardly be more complete than that between (*g*) and (*h*).

When the original trichosanthes solution is treated with strong hydrochloric acid the spectrum consists of three principal bands. The first band begins (penumbra) 1606, and extends to 1576; the maximum absorption is from 1593 (W. L. 608·6) to 1576 (W. L. 593·4). From this there is considerable absorption to the second band, of which the maximum absorption extends from 1557 (W. L. 577·6) to 1532 (W. L. 558·8). Slight absorption continues to the third band, which extends from 1497 (W. L. 536) to 1467 (W. L. 519). The relative intensities of these three bands are about 8, 7, 4. These three bands may perhaps represent the three first bands of the natural spectrum moved by different amounts towards the more refrangible end. When this spectrum (*i*) is compared with the spectrum of chlorophyll similarly treated (*j*), it is seen that the three bands correspond with three of the chlorophyll bands, while there is nothing to represent the first and most prominent chlorophyll band. It is of importance to notice, too, that the unrepresented chlorophyll band is the one least affected by reagents.

Another difference between the trichosanthes colouring matter and chlorophyll is that when treated either with ammonia sulphide or hydrochloric acid the former ceases to be green in thin layers, while the latter ceases to be red in thick layers. The reason for this becomes at once plain when the spectra are examined. The question of the relation which the trichosanthes colouring matter bears to chlorophyll is one of considerable interest, for it is evident that there is some relation, since in the products of the action of hydrochloric acid there are three bands which are common to the two spectra. That they differ greatly is equally evident from the dissimilarity of the original spectra and of those got after acting on the solutions with ammonia sulphide. The trichosanthes colouring matter is found inside a thick opaque rind, and so has probably been formed in the dark, at least it cannot serve the purpose which chlorophyll serves in leaves. Chlorophyll, according to Sachs, is formed in complete darkness only in the cotyledons of some conifers and in the leaves of ferns; but etiolin, which in darkness usually takes the place of chlorophyll, has a spectrum which does not materially differ from the chlorophyll spectrum, and is certainly not the same as that under discussion. Chlorophyll itself is held by many who have studied it to be a mixture of several distinct colouring matters. Professor Stokes * finds that the chlorophyll of land plants is a mixture of four substances, two green and two yellow, three of which are easily decomposed by acids. I cannot find any account of the experiments from which this conclusion is deduced, nor any description of the spectra of the several constituents, but it seems probable that the first band in the red is that due to the constituent which is not easily decomposed by acids. Mr Sorby † holds that the chlorophyll of land plants is a mixture of two substances, which he calls "blue chlorophyll" and "yellow chlorophyll." To each of these he ascribes a single absorption band in the red; but the diagram which accompanies his paper is so unsatisfactory that it is impossible to identify these with bands in the natural chlorophyll spectrum, though presumably the first band in the spectrum is that due to his "blue chlorophyll." But this first band is quite unrepresented in the trichosanthes spectrum, so that it seems a fair conclusion that the

* *Proc. Roy. Soc.* xiii. 144. † *Proc. Roy. Soc.*, xxi. 451.

"blue chlorophyll" of Sorby, or one, perhaps both, of the green chlorophylls of Stokes, is absent from the trichosanthes colouring matter. Its place is taken by another substance which yields the first band, which is not coincident with any of the chlorophyll bands. This substance, unlike that yielding the first chlorophyll band, is very readily decomposed by acids, and is slowly decomposed by ammonia sulphide.

Bands II., III., and V. in the trichosanthes spectrum apparently coincide in position with bands III., IV., and V. of the chlorophyll spectrum. Of these latter III. and V. are not distinctly shown in the accompanying plate, but are obtained with a comparatively weak solution and a strong light. Band IV. of the trichosanthes spectrum is extremely faint. It is not, however, possible to lay much stress on the apparent coincidence of these bands, since their behaviour under treatment with ammonia sulphide is so very different. A comparison of spectra (*g*) and (*h*) shows that no two of the bands are even approximately coincident—in fact, the two spectra are almost complementary. On the other hand, spectra (*i*) and (*j*) show that after treatment with strong hydrochloric acid, there is almost perfect coincidence between three of the bands in the two spectra. The influence of hydrochloric acid on chlorophyll has been studied by many observers. Russell and Lapraik * consider that the change produced is simply a molecular one, and call the two forms α- and β-chlorophyll. Schunck, on the other hand, holds † that the change is a chemical one, and that the ultimate product is identical with Frémy's phyllocyanin. The latter view certainly seems to explain the observations best, and in accordance with it we may say that the trichosanthes colouring matter contains a substance which is not chlorophyll, but which on treatment with hydrochloric acid yields one of the constituents of phyllocyanin, which is itself probably a mixture of two substances. In chlorophyll a partial change is produced by a very weak acid, so that extracts of certain leaves, such as those of the Virginian creeper, yield the modified spectrum, unless care be taken to neutralise the acid before extracting the chlorophyll. It is possible that a somewhat similar change has been produced in the trichosanthes colouring matter by the trichosanthin which occurs along with it in the pulp.

* *Jour. Chem. Soc.*, vol. xli. p. 334. † *Annals of Botany*, vol. iii. p. 86.

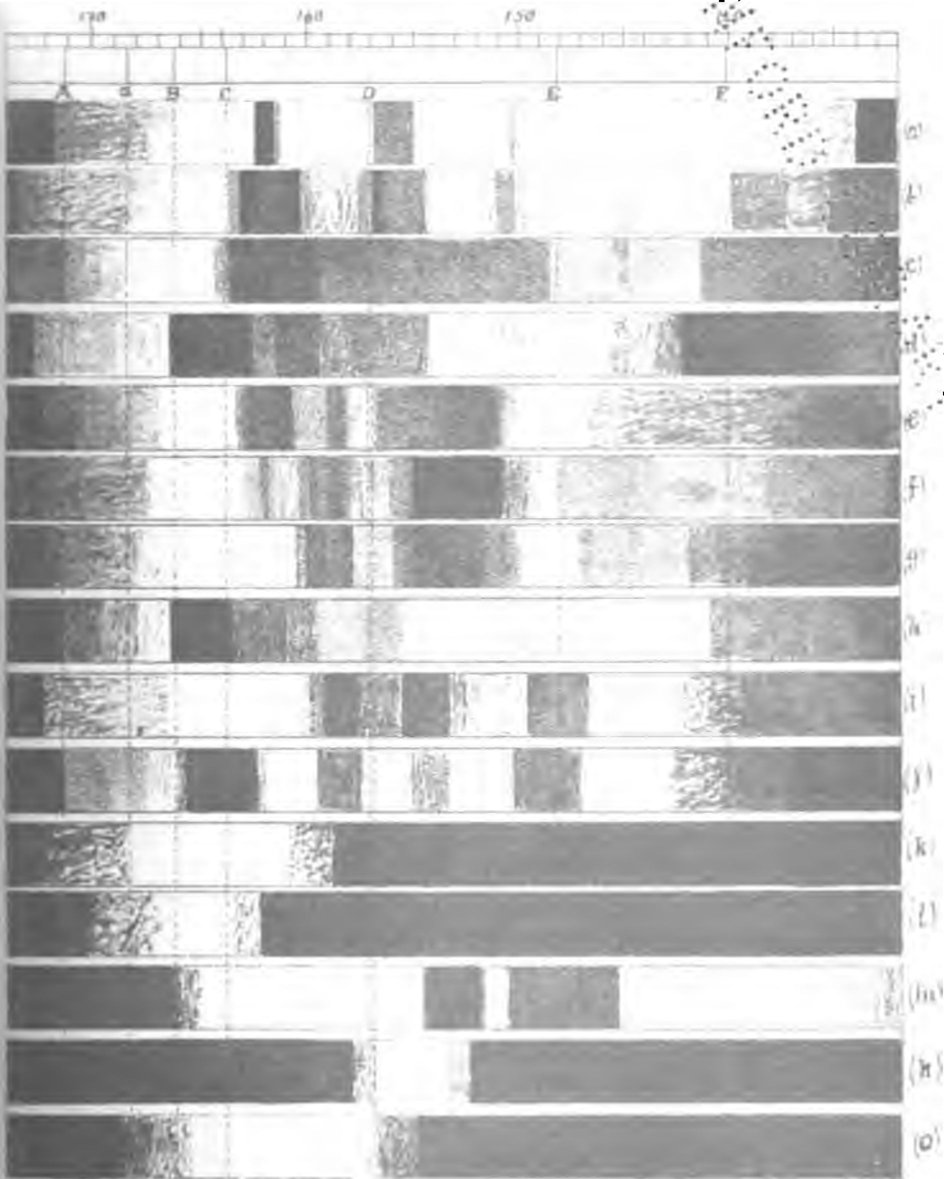
Colouring Matter from Ventilago madraspatana.—Of this Mr Hooper writes:—"The red colouring matter in the other bottle should be more extensively known. It is a drug that has been used as a dye in South India for ages, under the name of 'Vembádam bark.' It is the produce of a large creeper, the *Ventilago madraspatana*, which grows on the slopes of the Nilgiris and in the Mysore district. The bark, besides dyeing silk and other materials, yields its colour to fixed and volatile oils, and, what is a desideratum, to kerosene oil. From an analysis, I make it an anthracene derivative." The absorption spectrum for thin and thicker films is shown in figures (*k*) and (*l*). There are no absorption bands, but the light is completely absorbed, even with a very thin film, except from 1730 to 1588 (W. L. 604). With a thicker layer the absorption is complete, except from 1702 (W. L. 743) to 1622 (W. L. 636·6). Thus the absorption is almost entirely confined to the more refrangible end. This substance would probably be found very suitable for dyeing cloths for use as non-actinic blinds in "dark rooms."

Colouring Matter from the Funiculus of the Wattle.—This yields a very pretty spectrum, figure (*m*), characterised by two well-marked absorption bands. The first extends from about 1546 (W. L. 568·8) to 1519 (W. L. 550·2), the second from 1506 (W. L. 541·8) to 1453 (W. L. 511·2). The relative darkness of the two bands is 10 : 8. When a thicker layer of the substance is used the two bands almost come together, but a slight green colour is seen between them. The absorption between band II. and the end of the spectrum also increases. In an alkaline solution the bands are somewhat narrower, but their centres are unchanged in position.

Solution of "Waras."—The dye known as "Waras" is got from the glands of the fruit of *Flemingia Grahamiana* (W. and A.), one of the Leguminosæ. The absorption spectrum shows no bands, but absorption is complete except between 1580 (W. L. 597) and 1523 (W. L. 552·8).

Solution of "Kamala."—This dye is got from the glands of the fruit of *Rottlera tinctoria*, one of the Euphorbiaceæ. It resembles Waras very closely, but is transparent to light of a somewhat greater wave-length. The absorption is complete except between 1688 (W. L. 722) and 1549 (W. L. 571·2). The chemical relations of

ABSORPTION SPECTRA OF CERTAIN VEGETABLE COLOURING MATTERS.



Kamala and Waras have been examined by Mr D Hooper,* who shows that they are very similar, but not identical.

It is worth noting that of the colouring matters examined those giving definite absorption bands yield fugitive dyes, while the others yield permanent dyes.

My thanks are due to Dr J. Gibson for assistance received during the progress of the inquiry.

Description of Plate.

(*a*) Colouring matter from *Trichosanthes palmata*, 0·5 cm. thick.
(*b*) ,, ,, 2·0 cm. ,,
(*c*) ,, ,, 4·0 cm. ,,
(*d*) Chlorophyll from cabbage, 2 cm. thick.
(*e*) Colouring matter from *Trichosanthes palmata* + ammonia sulphide after 2 days.
(*f*) ,, ,, ,, 11 ,,
(*g*) ,, ,, ,, 39 ,,
(*h*) Chlorophyll + ammonia sulphide.
(*i*) Colouring matter from *Trichosanthes palmata* + hydrochloric acid.
(*j*) Chlorophyll + hydrochloric acid.
(*k*) Colouring matter from *Ventilago madraspatana*, thin layer.
(*l*) ,, ,, thick layer.
(*m*) ,, funiculus of Wattle.
(*n*) Waras.
(*o*) Kamala.

On a Mechanism for the Constitution of Ether. By Sir William Thomson.

(Read March 17, 1890.)

1. In a communication to the Royal Society of Edinburgh of 4th March 1889, I stated the problem of constructing a jointed model under gyrostatic domination, to fulfil the condition of having no rigidity against irrotational deformations, and of resisting rotation, or rotational deformation, with quasi-elastic forcive in simple proportion to rotation. I gave a solution, illustrated by a model, for the case of points all in one plane; but I did not then see any very simple three-dimensional solution. After many unavailing efforts I have recently found the following.

* *Pharmaceutical Journal*, vol. xviii. p. 213.

2. Take six fine straight rods, and six straight tubes, all of the same length, the internal diameter of the tubes exactly equal to the external diameter of the rods. Joint all the twelve together with ends to one point P. Mechanically this might be done (but it would not be worth the doing) by a ball-and-twelve-socket mechanism. The condition to be fulfilled is simply that the axes of the six rods and of the six tubes all pass through the one point P. Make a vast number of such clusters of six tubes and six rods, and, to begin with, place their jointed ends so as to constitute an equilateral homogeneous assemblage* of points P, P′ . . . . , each connected to its twelve nearest neighbours by a rod of one sliding into a tube of another. This assemblage of points we shall call our primary assemblage. The mechanical connections between them do not impose any constraint; each point of the assemblage may be moved arbitrarily in any direction, while all the others are at rest. The mechanical connections are required merely for the sake of providing us with rigid lines joining the points, or more properly rigid cylindric surfaces having their axes in the joining lines. Make now a rigid frame, G, of three rods fixed together at right angles to one another through one point O. Place it with its three bars in contact with the three pairs of rigid sides of any tetrahedron, (PP′, P″P‴), (PP″, P‴P′), (PP‴, P″P′) of our primary assemblage. Place similarly other similar rigid frames, G′, G″, &c., on the edges of all the tetrahedrons congener † to the one first chosen. The points O, O′, O″, &c., form a second homogeneous assemblage related to the assemblage of Ps, just as the reds are related to the blues in § 69 of the Article referred to in the footnotes.

3. The position of the frame G—that is to say, its orientation and the position of its centre O (six disposables)—is completely determined by the four points P, P′, P″, P‴ (Thomson and Tait's *Natural Philosophy*, § 198; or *Elements*, § 168). If its bars were allowed to break away from contact with the three pairs of edges of the tetrahedron, we might chose, as its six co-ordinates, the six distances of its three bars from the three pairs of edges; but we suppose it to be constrained to preserve these contacts. And now let any

* See "Molecular Constitution of Matter," §§ 45 (*a*) . . (*j*), *Proc. Roy. Soc. Edin.* for July 1889, to be republished as Art. XCVII. of Vol. III. of my Papers.

† *Ibid.*, § 13.

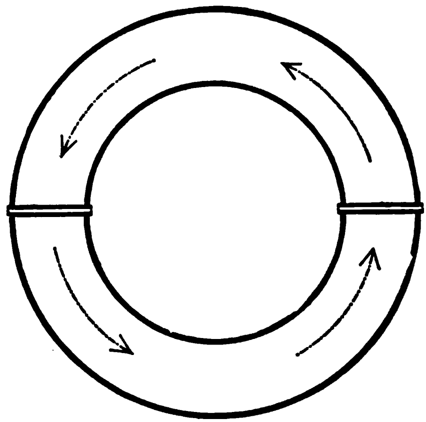
one of the points P, P′, P″, P‴, or all of them, be moved in any manner. The position of the frame G is always fully determinate. This is illustrated by a model accompanying the present communication, showing a single tetrahedron of the primary assemblage and a single G frame. The edges of the tetrahedron are of copper wire sliding into glass tubes. The wires and tubes are provided with an eye or staple respectively, through which a ring passes to hold three ends together at the corners. Two of the rings have two glass tubes and one copper wire linked on each, while the other two rings have each two copper wires and one glass tube.

4. Returning now to our multitudinous assemblage, let it be displaced by stretchings of all edges parallel to PP′, with no rotation of PP′, or P″P‴. This constitutes a homogeneous irrotational deformation of the primary assemblage. The frames G, G′, &c., experience merely translatory motions without any rotation, as we see readily by confining our attention to G and the tetrahedron PP′P″P‴. Consider similarly five other displacements by stretchings parallel to the five other edges of the tetrahedron. Any infinitely small homogeneous deformation of the primary assemblage (§ 1 above) may be determinately resolved into six such simple stretchings, and any infinitely small rotational deformations may be produced by the superposition of a rotation without deformation, upon the irrotational deformation. Hence an infinitely small homogeneous deformation of the primary assemblage without rotation produces only translatory motion, no rotation, of the G frames; and any infinitely small homogeneous displacement whatever of the primary assemblage, produces a rotation of each frame equal to, and round the same axis as, the rotational component.

5. It now only remains to give irrotational stability to the G frames. This may be done by mounting gyrostats properly upon them according to the principle stated in §§ 3–5 of Article C. of Vol. III. of my Papers, and in my Address to the Institute of Electrical Engineers, 10th January 1889. Three gyrostats would suffice, but twelve may be taken for symmetry, and for avoidance of any resultant moment of momentum of all the rotators of one frame. Instead of ordinary gyrostats with rigid fly-wheels, we may take liquid gyrostats as described below § 6, and so make one very small step towards abolishing the crude mechanism of fly-wheels and

axles and oiled pivots. But I choose the liquid gyrostat at present merely because it is more easily described.

6. Imagine a hollow anchor ring, or tore, that is to say, an endless circular tube of circular cross-section. Perforate it in the line of a diameter, and fix into it small straight tubes to guard the perforations as shown in the accompanying diagram. Fill it with frictionless liquid, and give the liquid irrotational circuital motion as indicated by the arrow-heads in the diagram. This arrangement constitutes



our hydrokinetic substitute for a mechanical fly-wheel. Mount it on a stiff diametral rod passing through the perforations, and it becomes the mounted gyrostat, or Foucault gyroscope, required for our models. Its use would have considerably simplified and shortened the description of a model communicated to the French Academy last September,* which, however, was given purposely in terms of the Foucault gyroscope because it thus describes real mechanism by which the exigencies of the model can be practically realised in a very interesting and instructive manner, as may be seen in §§ 3–5 of Article C. of my Papers, and in my Address to the Institute of Electrical Engineers of 10th January 1889.

7. Let XOX′, YOY′, ZOZ′ be the three bars of the G frame: mount upon each of them four of our liquid gyrostats, those on

* *Comptes Rendus*, 16th September 1889; Art. C. of Papers, Vol. III.

XOX′ being placed as follows, and the others correspondingly. Of the four rings mounted on XX′, two are to be placed in the plane of YY′, XX′, the other two in the plane of ZZ′, XX′. The circuital fluid motions are to be in opposite directions in each pair.

8. The gyrostatic principle stated in § 5 of Art. C. of Vol. III. of my Papers, applied to our G frame, with the twelve liquid gyrostats thus mounted on it, shows that if, from the position in which it was given with all the rings at rest, it be turned through an infinitesimal angle i round any axis, it requires, in order to hold it at rest in this altered position, a couple in simple proportion to i; and that this couple remains sensibly constant, as long as the planes of all the gyrostats have only changed by very small angles from parallelism to their original directions. Hence, with this limitation as to time, our primary homogeneous assemblage of points, controlled by the gyrostatically dominated frame, G, G′, &c., fulfils exactly the condition stated for the ideal ether of § 14 of Art. XCIX. of Vol. III. of my Papers, which is as follows:—It has no intrinsic rigidity, that is to say, no elastic resistance to change of shape; but it has a *quasi*-rigidity, depending on an inherent quasi-elastic resistance to absolute rotation. It is absolutely non-resistant against change of volume and against any irrotational change of shape. Or it is absolutely incompressible. The model may be made so by introducing struts between the points P of the primary assemblage and their nearest neighbours O, O′, &c., of the G frames according to § 70 of "Molecular Constitution of Matter" (*Proc. Roy. Soc. Edin.*, July 1889).

9. If the velocity of the motion of the liquid in each gyrostat be infinitely great, each G frame exerts infinite resistance against rotation round any axis; and if the bars and tubes constituting the edges of the tetrahedron, and the bars of the G frames were all perfectly rigid, the primary assemblage is incapable of rotation or of rotational deformation; but if there is some degree of elastic flexural yielding in the edges of the tetrahedron, or in the bars of the G frame, or in all of them, the primary assemblage fulfils the definition of § 9, without any limit as to time, that is to say, with perfect durability of its quasi-elastic rigidity.

10. A homogeneous assemblage of points with gyrostatic quasi-rigidity conferred upon it in the manner described in §§ 2–8 would,

if constructed on a sufficiently small scale, transmit vibrations of light exactly as does the ether of nature. And it would be incapable of transmitting condensational-rarefactional waves, because it is absolutely devoid of resistance to condensation and rarefaction. It is, in fact, a mechanical realisation of the medium to which I was led one and a half years ago,* from Green's original theory, by purely optical reasons, in endeavouring to explain results of observation regarding the refraction and reflection of light.

On the Swimming Bladder and Flying Powers of *Dactylopterus volitans*. By W. L. Calderwood. *Communicated by* Professor Ewart. (With Plate.)

(Read February 17, 1890.)

Dactylopterus volitans, the so-called "flying gurnard," is not included by Günther in the genus Triglidæ, as its name might imply, but is assigned to the small allied family of Cataphracti.

Günther diagnoses it as follows :—"Dactylopterus, no lateral line, pectoral fins very large, an organ of flight, with the upper portion detached and shorter, granular teeth in the jaws, none on the palate, air bladder divided into two lateral halves, each with a large muscle."

Before proceeding to describe in detail the swimming bladder and other anatomical peculiarities, it may be well to state, that the skull is provided with a superficial bony covering which projects backwards over the region of the "shoulder" in two flattened plates, each terminating in a spine (seen in fig. 2). Also that the first four vertebræ of the column have coalesced so as to form a rigid tube, the neural spines being united as a vertical plate, which for convenience I have termed the neural plate.

The *swimming bladder*, so far as I am aware, has a unique position, since it is situated not below, but above the vertebral column, not forming part of the abdominal contents, but situated dorsally in a special cavity of its own. The appearance of the

* *Philosophical Magazine*, Nov. 1888, "On the Reflection and Refraction of Light," by Sir W. Thomson.

swimming bladder in Trigla is well known; there, on opening the abdominal wall, it is distinctly seen projecting downwards from the dorsal wall of the abdominal cavity. In Dactylopterus, when the viscera are removed, only the *ventral* surface of the bladder is seen forming a part of the dorsal boundary of the abdominal cavity. This portion is white and tendinous, having on each side the commencement of a powerful muscle (fig. 1). The kidneys overlap it posteriorly, and the œsophagus and pericardial cavity partially obscure its anterior end. On removing the dorsal muscles of the trunk so as to view the bladder from the side and back, the bladder muscles are seen to continue upwards, to curve inwards towards the median line, to be reflected downwards on each side of the neural plate, and finally to become attached to the bodies of the vertebræ, whose spines go to form that plate. In this way the bladder becomes divided longitudinally into its two lateral portions. A careful examination, however, reveals more than this. On tracing the dorsal surface forwards by removing the superficial backward prolongation of the skull-sheath, a secondary division of the bladder is disclosed. It is composed of an extremely thin transparent membrane, is triangular in shape, its dorsal surface being so superficial as only to be covered by the dorsal plate of the shoulder. The base of the triangle is towards the median line, the other two sides are surrounded by bone, the ventral surface also lies in a bony cup; so that this whole portion, although composed of an extremely thin membrane, is completely protected by rigid surroundings. Moreover, one-half of it may be fairly said to be within the region of the skull, since a section taken through the plane of the foramen magnum would, as nearly as possible, bisect the triangle. As seen in fig. 2, the posterior margin is the only one not surrounded by bone. Here it is that the primary division of the bladder communicates with this smaller secondary portion, that the muscular coat of the one thins out to give place to the thin membrane of the other. Making now a vertical section through the long axes of the primary portion on one side, so as to obtain a view of the interior, we see that there is a thin membrane dividing it transversely into two—that in this membrane there is a foramen situated towards the inner margin. Immediately behind this membrane there is a tunnel seen passing in a transverse manner below the vertebral column, forming

a channel of communication between the primary portions of each side. This is the only part of the bladder which is situated below the level of the vertebral column.

The entire bladder, therefore, instead of being composed of "two lateral halves," is in reality made up of six parts,—on each side a primary portion divided into two by a membrane, and anterior to and above this a secondary portion.

Günther, in his *Introduction to the Study of Fishes*, tells us that the air bladder and the organ of hearing are connected in a more or less simple way in the perch and herring, and by means of a complicated chain of ossicles in Siluridæ, Characinidæ, Cyprinidæ, and Gymnotidæ. The details of the Siluroid Amiurus have been worked out by Ramsay Wright;* and T. Jeffray Parker has written "On the Connection of the Air Bladder and the Auditory Organ in the Red Cod (*Lotella bacchus*)." † Taking these data into consideration, and the fact that part of the swimming bladder of Dactylopterus is produced into the region of the head, I thought that we might have, in this instance, a similar connection. My most careful dissection revealed nothing but the anterior portion of the bladder ending blindly in a rigid boundary of bone. Similarly, I might mention that I could discover no connection between the bladder and alimentary tract,—no pneumatic duct. The amount of gas present in the bladder must therefore depend on six retia mirabilia, which, horse-shoe shaped, are arranged, three in each primary division of the bladder,—two ventrally and one dorsally.

A. Moreau, in his work *Sur la voix des poissons*, describes an interesting experiment which he made on *Trigla hirundo*. Observing two nerves passing to the air bladder, having their origin below the pneumogastric near to the first dorsal pair, he stimulated them with electricity and produced the characteristic grunting sounds. On studying the bladder, he found in the dividing septum (which he calls the diaphragm) both radiating and circular muscular fibres, forming a sphincter round the central foramen. On removing the posterior end of the bladder and stimulating again, although no sound was heard, the sphincter was seen to contract. In the entire bladder,

* *On Anatomy of Amiurus*, by Ramsay Wright, M'Murrich, Macallum, and M'Kenzie.

† *Trans. N.Z. Instit.*, vol. xv. p. 234, 1882.

if the wall only was stimulated, no sound was produced except when the current was greatly intensified. He therefore concludes that the sound is produced by vibrations caused by the contractions of this diaphragm.

In Dactylopterus, while killing two of the four specimens examined, sounds exactly similar to those of the gurnard were distinctly heard, and by holding the fish between the finger and thumb in the region of the swimming bladder, a distinct contraction of the bladder could be felt as each sound was produced. These movements and sounds were quite independent of any movements of the operculum or mouth.

I examined this bladder-diaphragm in *Trigla hirundo,* also in *Trigla lyra* and in Dactylopterus. In all three the sphincter is present. Arguing from the fact that all gurnards produce these sounds, and that they all have perforated diaphragms, whereas a perforated diaphragm is, so far as I am aware, not found in any fish which does not produce the sounds, I am inclined to support the conclusions come to by Moreau.

The *flying powers* of Dactylopterus are, I find, by some called into question. The conservator of the Naples Zoological Station —where these observations were made—insisted that the fish never left the water. I have been unable to make any observations on the actual flight myself, but the literature on the subject seems to me to be conclusive. Möbius,* in speaking of the flight of fish, discusses the question as to whether Dactylopterus moves the wings. This he does from direct observation.

Moseley † twice describes the flight. In the second instance, while collecting in a boat amongst the weed of the Sargasso Sea, he succeeded in capturing one or two in a hand-net. They flew, he says, "at a height of about a foot above the water, for distances of fifteen or twenty yards."

Comparing Dactylopterus with the well-known flying fish Exocœtus, the "wings" are, in proportion, quite as large, in each case reaching, when drawn close to the side, to the base of the caudal fin. In the former, the first six rays are detached from the rest of the fin, bent downwards, and are soft at their extremities. The

* Möbius, K., *Die Bewegungen der fliegenden Fische durch die Luft.*
† Moseley, *Notes by a Naturalist on the "Challenger,"* pp. 562 and 571.

pectoral muscles are powerful. They are arranged in a superficial and a deep layer, both above and below the fin, and are in pairs. In the upper pair, the superficial elevates, and at the same time protracts; the deep adducts, sweeping the fin in to the flank. The fin rays are devaricated from one another by tendinous fibres passing across the upper surface obliquely from the anterior portion of the superficial protractor. The detached portion of the fin is only very slightly acted upon by fibres from above.

In the lower pair, both divisions of the fin are drawn forwards and downwards by fibres from both muscles, the superficial, however, which is coarsely fasciculated, having the stronger action upon the large portion of the fin. The detached portion, indeed, has an extremely limited motion, and always must stand out at an angle from the body of the fish. The swimming bladder of Exocœtus occupies the ordinary position in the abdominal cavity and is non-muscular. This fish, I have no doubt, is capable of flying a much greater distance than Dactylopterus; it has much less mass, and in form is calculated to offer much less resistance to the air, and more readily to retain its natural position, when out of the water, owing to its deep-keeled herring body. The significance of the peculiar swimming bladder in Dactylopterus now seems to me to be explained.

The true gurnards, that is, the near allies of the Cataphracti, are in habit bottom feeders. The tips of the free fin rays are provided with isolated sensory elements,* causing these portions to have a function analogous to the antennæ of crustaceans. Their swimming bladders have no pneumatic duct and no muscle. When brought suddenly to the surface by means of a net or line, Trigla invariably floats belly up. If the fish is then opened, the bladder is found to be violently expanded. The expansion naturally takes place in a downward direction amongst the abdominal viscera, this being the line of least resistance. The balance is in this way destroyed beyond recovery, and the position of the fish is reversed. I have kept *Trigla cataphracta* in a tank in this condition for two days. The fish eventually dies. The reason of the expansion appears to me to be the sudden removal of pressure consequent upon the fish being brought

* Zincone, A., *Osservazioni anatomiche su di alcune appendici tattili dei pesci*, 1876.

rapidly to the surface. Dactylopterus is, I believe, an example of a fish which can readily adapt itself either to a bottom life or to a more than pelagic one. It has a number of the gurnard characteristics, the free fin rays, soft and projecting downwards, the flattened head and ventrally protruding mouth, the heavy-looking rounded body. I am totally ignorant of any animals which prey upon Dactylopterus, but let us suppose that for some reason or other it becomes necessary for it to leave the bottom and take to the air, then the sudden expansion of bladder seen in the gurnard is prevented by the action of the strong bladder muscles. In addition to this, its air cavity has the highest possible dorsal position, as has been already shown, which must be of great service in enabling the fish to keep its somewhat unwieldy body in its natural position. The thin-walled secondary portion of the bladder can, of course, not be expanded owing to its complete envelopment in bone. From a study of the young Dactylopterus, *i.e.*, the fish formerly known as Cephalacanthus, it is evident that only the adult forms can have the power of "flight." The pectoral fins of young examples are quite insignificant as flying organs.

The smallest specimen I examined was $1\frac{3}{4}$ inches. Here, the pectorals, when placed along the side, reached only to the level of the second dorsal fin ray. The swimming bladder was as in the adult. I was unable to determine the period at which the fish first ventured into the air, but gauging by the development of the pectoral fins, it may probably be when it has attained the length of about 6 inches.

In the skull the encephalic arch is the only one which requires our attention. A strong solution of caustic potash is necessary to loosen the bony sheath of the head so that the superficial plates may be removed. From the basioccipital the neural arch slopes forwards, and is modified in a peculiar way, so as to make room for the secondary portion of the swimming bladder. The exoccipitals send processes backwards curving at an angle and enclosing this secondary portion, and the paroccipitals take their position immediately in front. The processes of the exoccipitals are attached to the dorsal superficial plate, and flatten out posteriorly under the inner suture of that plate. Each exoccipital, with its process, in this way forms the outer margin of the secondary portion of the bladder. The

floor of the cavity is formed by the same bone. Each paroccipital is responsible for part of the anterior end. The supraoccipital and part of the parietal make up the remainder of the end. The inner margin is formed by what I have called the neural plate, *i.e.*, the coalesced neural spines. The roof is formed by the dorsal sheathing plate.

It has already been noticed that in the vertebral column, the first four vertebræ have coalesced so as to form a simple tube. This region of the body, therefore, taking into consideration this tube and the dorsal bony plates, exhibits a peculiar rigidity, which must be highly serviceable to the fish if my explanation of the unique position of the swimming bladder is correct. A similar condition of vertebral column is seen in Fistularia (one of the pipe fishes), but here, so far as I can find out, the bladder is similar to that seen in the other members of the family to which it belongs; it is, viz., a simple, non-muscular bladder, depending in the ordinary way from the dorsum of the abdominal cavity. This condition, therefore, although closely associated with the swimming bladder in the case of Dactylopterus, may not be developed on account of it, or in adaptation to its peculiarities. In cross section the tube takes much the outline of a cervical vertebra without transverse processes. The lower surface is, along the median line, formed into a groove, down which passes the dorsal aorta. The eminences on the sides of this groove are hollowed out into canals, which, as it were, tunnel through that part of the tube to which the bladder muscles are attached, emerging to the exterior of the tube before and behind the muscle. In Dactylopterus we have both a body and a head kidney, and the renal veins, in passing from the former to the latter, occupy the two canals described above. The kidneys call for treatment in greater detail. This will form the subject of another paper.

W. L. CALDERWOOD ON DACTYLOPTERUS VOLITANS

Fig 2.

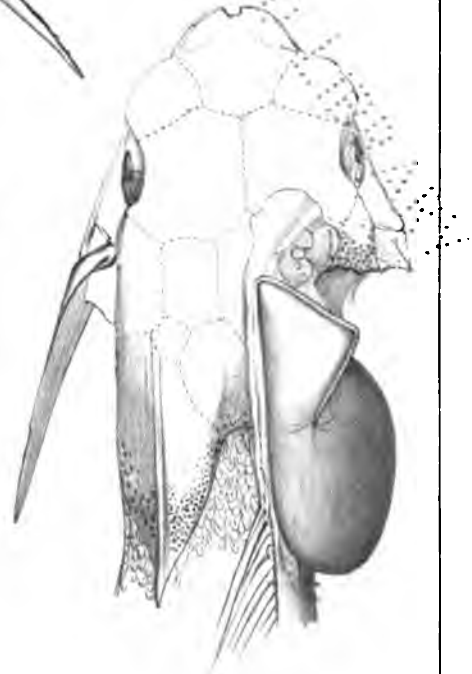


Fig. 1.

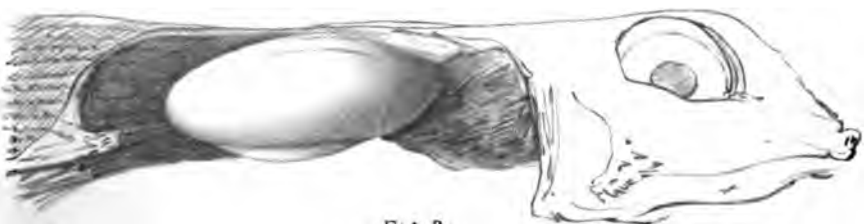


Fig. 3.

W L C del ad nat.

McFarlane & Erskine Lith^rs Edin^r

Notes on the Solution of certain Equations. By R. E. Allardice, M.A.

(Read April 7, 1890.)

The equations considered are equations of the third and fourth degree in a single variable, and systems consisting of two equations in two variables. This is a comparatively small class; but if there be added systems consisting of a single equation of the third or fourth degree and a number of others all of the first degree, it will include all equations which admit, in general, of an algebraic solution.

The object of these notes is to point out the advantage of making greater use, than is generally the custom, of the discriminant in the solution of equations, and to emphasise the importance of looking for geometrical illustrations of analytical methods whenever this is possible. The usefulness of such illustrations is well known in considering, for example, limiting cases in the solution of equations, such as the cases of infinite roots and of equal roots, of the meaning of which it is almost impossible to form an adequate conception, without the use of such illustrations.

Consider the system

$$\left.\begin{aligned} U &\equiv ax^2 + 2hxy + by^2 + 2gx + 2fy + c = 0 \\ V &\equiv a'x^2 + 2h'xy + b'y^2 + 2g'x + 2f'y + c' = 0 \end{aligned}\right\} \quad . \quad . \quad . \quad (A).$$

This system may be solved algebraically by eliminating one of the variables, say y, and obtaining a biquadratic in x. This biquadratic may in particular cases be soluble by means of quadratics; but, as a rule, it will require the general method of solution, by means of the reducing cubic. It will, however, in almost every case be found simpler to determine k so that $U + kV$ shall resolve into factors. The equation in k will be of the third degree. If the solution of the system (A) can be made in any way to depend on quadratics alone, the cubic in k must have at least one rational root, which can easily be obtained in every case; whereas the biquadratic in x will in general have no rational root at all. In the general case, when the solution of the system (A) cannot be made to depend on quadratics alone, the use of the cubic in k will save the trouble

of eliminating y from the two equations. It will be found that the cubic in k is identical with the reducing cubic of the biquadratic in x.

As an illustration, consider the so-called homogeneous system

$$\left.\begin{aligned} U &\equiv ax^2 + bxy + cy^2 + d = 0 \\ V &= a'x^2 + b'xy + c'y^2 + d' = 0 \end{aligned}\right\}.$$

This system may be solved by putting $y/x = v$, and obtaining a quadratic in v. This method of solution is an algebraical interpretation of the fact that the points of intersection of two concentric conics are the vertices of a parallelogram. The above system may also be solved by making use of the fact that $-d/d'$ is one value of k for which $U + kV$ may be resolved into factors; and this solution is practically the same as that obtained by putting $y/x = v$. The question then naturally arises, what are the other two values of k which make $U + kV$ resolvable into factors? These values are the values for which $(ax^2 + bxy + cy^2) + k(a'x^2 + b'xy + c'y^2)$ is a complete square; and this suggests another method of solution.

By way of further illustration, a method of obtaining the reducing cubic of the biquadratic $x^4 + px^2 + qx + r = 0$, may be noticed.

The resultant in x of the two equations

$$\left.\begin{aligned} &x^2 - y + p/2 = 0 \\ &y^2 + qx + r - p^2/4 = 0 \end{aligned}\right\} \text{is } x^4 + px^2 + qx + r = 0 .$$

Now the equation to determine k so that

$$(x^2 - y + p/2) + k(y^2 + qx + r - p^2/4)$$

may be resolvable into factors is

$$k^3 - 2pk^2 - (4r - p^2)k + q^2 = 0 ,$$

which is the reducing cubic of

$$x^4 + px^2 + qx + r = 0 .$$

[The resultant in y is

$$y^4 + (2r - p^2/2)y^2 - q^2y + (p^4/16 + r^2 - p^2r/2 + pq^2/2) = 0 ,$$

the reducing cubic of which is

$$k^3 - (4r - p^2)k^2 - 2pq^2k + q^4 = 0 ,$$

which may be transformed into the above reducing cubic by dividing the roots by q^2 and then forming the reciprocal equation.]

As a last illustration consider the system *

$$x^2+y=a, \qquad y^2+x=b.$$

The resultant in x is

$$x^4-2ax^2+x+a^2-b=0 \quad . \quad . \quad . \quad . \quad . \quad (1).$$

Now in the general biquadratic (deprived of its second term) $x^4+px^2+qx+r=0$, put $x=\lambda y$; then

$$y^4+(p/\lambda^2)y^2+(q/\lambda^3)y+r/\lambda^4=0.$$

Now put $q/\lambda^3=1$, that is $\lambda=q^{1/3}$, and the equation becomes

$$y^4+p'y^2+y+r=0, \text{ where } p'=p/\lambda^2=p/q^{2/3},\ r'=r/q^{4/3}.$$

This may be identified with (1) by putting

$$-2a=p' \text{ and } a^2-b=r'; \text{ that is } a=-p'/2,\ b=p'^2/4-r'.$$

Hence the general biquadratic may be solved graphically by means of the two parabolas $x^2+y=a$ and $y^2+x=b$.

The advantage of this method is that these two parabolas only vary as regards position, a change in a causing the first parabola to move along the y-axis, and a change in b the second parabola to move along the x-axis. In applying this method in practice one of the parabolas might be drawn permanently on a sheet of paper ruled in squares; and instead of shifting the parabola it would be sufficient to change the position of the origin of coordinates. The second parabola would require to be drawn on a sheet of tracing-paper or engraved on glass.†

The above method fails when the roots are equal in pairs, for in this case $q=0$. This is also evident geometrically, since two parabolas cannot have double contact unless their axes are parallel. Three roots of the given biquadratic will be equal when the two parabolas have contact of the second order. The point of contact will obviously lie on the line $y=x$, and the tangent at the point of

* The idea of using this system of equations for obtaining a graphical solution of the general biquadratic is due to Professor Chrystal.

† When this paper was read a diagram was shown illustrating the graphical solution of several biquadratics.

contact will be equally inclined to the axes; and from these facts the conditions are easily found to be $a = b = 3/4$.

In the cubic $x^3 + q'x + r' = 0$, put $x = \lambda y$, then

$$y^3 + (q'/\lambda^2)y + (r'/\lambda^3) = 0.$$

Put $q'/\lambda^2 = \pm 1$, that is $\lambda = (\pm q')^{1/2}$, and the equation reduces to $y^3 \pm y + r = 0$. This may be solved graphically by means of the fixed curves $y^3 = x$ and the straight line $x \pm y - r = 0$.

Notes on the Zodiacal Light. By Prof. C. Michie Smith.

(Read April 7, 1890.)

So much has been written on the zodiacal light that it may appear rather rash to say that a really accurate examination of this phenomenon has yet to be made. Still I have no hesitation in saying that neither its position, its shape, nor its spectrum has yet been determined with sufficient accuracy. In the hope of being able to add something to our knowledge of the last of these, I obtained, in 1882, a grant from the Government Grant Fund for the construction of a spectroscope specially designed for observing and, if possible, photographing the spectrum of the zodiacal light. The apparatus, which was made by Mr A. Hilger, consists of two interchangeable collimators of 36 inches and 8 inches focal length and 1½ inch aperture; an Iceland spar prism 1¼ inch high, 2¼ inches side, a camera with a lens of 8 inches focal length, and an observing telescope and a heavy glass prism which can be used for eye observations. The camera and telescope are fixed to a common base movable about a pivot concentric with the pillar carrying the prism, so that they are readily interchanged. The lenses are all of quartz, and the dark slide carrying the sensitive plate can be placed at such an angle that the whole or at least a considerable part of the spectrum is in focus at the same time. The slit is provided with shutters for exposing any required portion alone, and the jaws of the slit both move equally, so that the centre remains fixed in position. A photographed scale can be thrown in in the ordinary way, but I have found this useless when dealing with faint spectra, since any illumination which will render the divisions visible quite obliterates the spectrum. After trying various plans I finally

adopted the following, which is probably not new, though I have never seen it described. The photographed scale is removed from the end of its tube, and is replaced by a small slit with adjustable jaws. The tube is then mounted on a vertical axis concentric with a fixed horizontal, graduated circle. The tube is rigidly attached to the arm carrying the verniers of this circle. When the position of any line has to be determined, the slit is adjusted so as to admit just sufficient light to render its reflected image visible, and the tube is rotated till this image coincides with the line. It is of course easy to adapt Prof. C. P. Smyth's illumination to this arrangement.

The attempts so far made to photograph the zodiacal light spectrum have been total failures, but I hope that the use of plates with a greater range of colour sensitiveness may yet prove successful.

The spectrum as usually seen is extremely faint, so that the observer ought to prepare himself for his work by remaining in a dark room for at least ten minutes before he attempts to make observations, but if this precaution be taken the spectrum can be seen on any clear night when there is no moonlight and neither Jupiter nor Venus is very near the place of observation. As ordinarily seen, very little colour can be distinguished in the spectrum, but under favourable conditions a distinct tinge of red can be observed. Except on a few occasions the spectrum, as I have seen it, was continuous and quite free from bright lines, but on several nights in 1883 I saw what appeared to be a bright line. The following extracts from my note-book give all the cases of this appearance.

1883. *March 7th.*—Spectrum in direct vision spectroscope distinct up to at least 45° altitude: apparently continuous, but with part more brilliant than the rest.

March 8th.—Spectrum clearly visible. The most notable feature is the rapid variations in brightness. There seems to be either a bright band or a part of maximum brightness somewhat like that seen on the more refrangible side of the D line towards sunset. (Observation made with long collimator and glass prism.)

March 28th, 7^h 20^m to 7^h 50^m P.M.—Short collimator. Light faint and very diffuse: no clouds: eight stars easily visible in Pleiades. Spectrum flickering and faint, but shows almost certainly a bright

line, apparently in the yellow-green, but position doubtful, as only a rough method of comparison was available.

March 29th, 7.30 to 8 P.M.—Short collimator, low power eye-piece. Night fine: no clouds. Continuous spectrum, brighter part beginning in the yellow. Bright line appeared to flash out at times, but very doubtful.

April 2nd.—Clouds near horizon. Bright continuous spectrum, which ended sharply near D. A place of maximum brightness near centre. Flickering.

April 5th.—Direct vision spectroscope. Clouds near horizon. Light faint, but spectrum fairly bright. There appeared to be two bright lines. At times the lines seemed quite distinct, at other times quite invisible.

April 24th, 7.30 P.M.—Short collimator, high power eye-piece. Light clouds and a slight haze near the horizon, through which the Pleiades were dimly visible. Z. L. not very bright, but extended through at least 60°. Spectrum pretty bright, and bright line appeared to flash up near the red end. Position determined by bringing end of faintly illuminated scale to coincide, and then comparing solar lines with the scale on the morning of the 25th, everything having been left unchanged.

The position thus determined corresponded to a wave-length of 558. After this the spectroscope was fitted for photographic work, and plates were exposed on April 30th and May 1st; on the former for an hour, on the latter for an hour and a half, but without any result. The following is the note for the latter of the two dates:—

May 1st.—Z. L. very bright but very diffuse, rising well above Jupiter, near which it seemed to bifurcate. Slight haze about horizon, but stars visible through it down to the horizon. Spectrum as seen with eye-piece held behind camera very bright, and extended to the red. With direct vision spectroscope very bright at first, but soon getting much fainter. The line at times seemed to flash out. At other times the spectrum seemed to be banded.

Since then I have never seen the least trace of a line, though on a number of occasions I have had particularly good opportunities of examining the spectrum, especially in January 1885, when I spent the first twelve days of the year on the top of Dodabetta, 8642 feet

above mean sea-level. The notes made on that occasion include the remark "certainly no bright lines."

It appears, then, that in all my observations, which have been carried on at intervals since 1875, the spectrum has appeared continuous and free from bright lines except during the spring of 1883, and that even then the lines were not seen with sufficient distinctness to make their existence certain. The estimated position of the supposed line, W. L. 558, differs but little from that of the auroral line (W. L. 556·7) which was observed by Ångström* in the zodiacal light spectrum in 1867. He, however, was observing at Upsala, where the auroral spectrum can often be seen in almost all parts of the sky, even when the aurora itself cannot be detected. Senor A. T. Acrimis,† observing at Cadiz, saw two bright lines in the spectrum, but since he used a refractor and a five prism spectroscope it is almost certain that it was not the spectrum of the zodiacal light that he saw. There would seem to be very little risk of obtaining the auroral spectrum in Madras, and I think that if the bright line seen was real and not imaginary it must have been due to the zodiacal light. It seems, too, to be in favour of its reality that my own prepossessions—arising from hundreds of previous observations, were entirely against the existence of such a line. These observations are presented to the Society with great hesitation, but as they were made with every precaution that I could devise, I think it best to place them on record.

Another point with reference to the zodiacal light on which more detailed and accurate observations are much needed is its exact position in the sky. It was only recently that I discovered how very unsatisfactory existing determinations were, and I do not propose at present to give any results of my own measurements. I wish, however, to call attention to one or two points. Very little weight can be laid on determinations of the inclination of the axis of the light made in high latitudes, for Searle has shown‡ that the difference in absorption between the upper and lower boundaries may produce a very considerable displacement of the apparent axis. But no such explanation will account for the differences shown in

* *Pogg. Annal.*, vol. 137, p. 162.
† *R. A. S. Monthly Notices*, xxxvi. p. 1.
‡ *Proc. Amer. Acad.*, vol. xix. p. 146 ; *Memoirs Amer. Acad.*, xi. p. 135.

existing records. Take, for example, those made by Captain Jacob in Madras in 1856–57–58 compared with those of Captain Tupman in the Mediterranean in 1869–71.

The former * found that the axis of the light was inclined but slightly to the ecliptic, and that the extreme positions of the vertex of the western cone were lat. 2° S. and lat. 6° N., while the latter states † that the inclination to the ecliptic was often as much as 20°, and that an error of 3° or 4° in his observations is inadmissible.

It is extremely difficult to fix the position of the light by any of the ordinary methods, but I have hopes of getting a very fair estimate of the inclination of the axis by using a form of clinometer. This consists of a stand with levelling screws carrying a long wooden arm movable in a vertical plane about an axis at one end, and provided with an arrangement for clamping it at any required angle. The angle is measured by means of a plumb line suspended from the upper end. I find it possible to place this arm parallel to the axis of the light with considerable accuracy. At the same time it seems probable, as remarked by Searle, that no really satisfactory determination of the position of the light will be obtained except by a careful photometric survey. A method of making such a survey has, however, still to be devised, and to be satisfactory it would have to be preceded by a photometric survey of the region in which the light falls.

On the Structure and Contraction of Striped Muscular Fibre of the Crab and Lobster. By Professor William Rutherford, M.D., F.R.S.

(Read February 28, 1890.)

(*Abstract.*)

The author gave an account of the microscopical appearances of striped muscle of the crab and lobster. The muscle of the crab and lobster is suitable for investigation because of the comparatively large size of the structural elements, and the readiness with which the sarcous matter can be fixed and otherwise prepared in different con-

* *Memoirs R. A. S.*, vol. xxviii. p. 119.
† *R. A. S. Monthly Notices*, xxxi. p. 74.

ditions. The author is entirely opposed to the opinions expressed by Melland, and more recently by Gehuchten, regarding the structure of the sarcous matter, and maintains, as he did at the International Medical Congress in 1881 (*Transactions International Medical Congress*, 1881, vol. i. p. 270), that the sarcous matter essentially consists of contractile fibrils, with an interstitial substance between them—an opinion previously expressed by Kölliker and others, and recently supported by Rollett. Fibrils are the contractile elements in non-striped muscle. Fluid is contained in the interstices of the invisible micellar network of their seemingly homogeneous protoplasm. The shortening of the fibrils doubtless implies a change in the relative positions of the micellæ in the networks, but there is no evidence of any shifting of fluid from one part of the fibril to another. The fibrils of striped muscle are segmented, and one of the events of contraction is the shifting of fluid from one segment to another. Each fibril consists of segments arranged in linear series in regular alternate order. *Bowman's element* is the longest segment, and appears to be the only one that is really contractile. Its dimness is due to a substance resembling myeline enclosed in a contractile tissue. There is a node in the equator of Bowman's element, the position of which is sometimes marked by a dim line described by Hensen, but the author finds no evidence of any transverse membrane there as described by some authors. Between the ends of Bowman's elements there is a segment about half the length of Bowman's element, termed by the author the *intermediate segment.* It is a tissue containing a watery fluid, and there is a globule of myeloid substance definitely located in the equator of the segment, and marking the position of a node; the author finds no evidence of a transverse membrane there, such as Krause and others have described. Myeloid substance also sometimes occurs throughout the shaft of the intermediate segment, but always in smaller amount than in Bowman's element. The lateral coaptation of the central globules in neighbouring intermediate segments produces the line to which attention was first particularly directed by Dobie in 1848. The intermediate segments appear not to be contractile, and probably serve as elastic buffers between the ends of Bowman's elements when they approach each other during contraction. A third segment, termed by the author the *proper*

clear segment, is seen between Bowman's element and the intermediate segment, when the uncontracted fibril is stretched to its full physiological length. It is almost quite clear, and appears to consist of a thin envelope containing a watery fluid, and a particle of myeloid substance belonging to the granule line described by Flögel as crossing the fibre in the light stripe. The whole fibril has a thin envelope.

The first event of contraction consists in a shortening of the interval between the ends of Bowman's elements, which in their approximation come close to the globule of Dobie's line. The shortening appears to result from an active absorption of fluid from the clear and intermediate segments by Bowman's elements. In the second stage of contraction the fibril seems "homogeneous," unless it is suitably stained and sufficiently magnified. In this so-called "homogeneous" stage the ends of Bowman's elements are in close proximity to the globule of Dobie's line, which is now somewhat flattened; the myeloid substance has not yet begun to shift its place in Bowman's element. In the third stage of contraction the myeloid substance moves away from the shaft on each side of the equatorial node in Bowman's element, accumulates in its ends, and the element shortens, owing to a real contraction of its tissue. There is a "reversal of the stripes" in the contracted fibre, as Flögel first pointed out. The dim stripe of the contracted fibril consists of the approximated ends of two Bowman's elements with the myeloid globule of the intermediate segment between them, now much flattened, owing to lateral extension by the thickening of the contracted fibril. The light stripe of the contracted fibril consists of the shaft of Bowman's element that has become clear, owing to shifting of the myeloid substance to the ends of the element. The myeloid substance appears to be completely moved to the ends of Bowman's elements only when their shafts contract and squeeze it out; but it begins to move out of the shafts before they contract. The fluid absorbed by the element probably passes into the interstices of the micellar network; the myeloid substance appears to be contained in a special set of spaces. The contraction of the micellar network does not express the absorbed fluid, which therefore does not leave Bowman's element until its contraction is over. The elasticity of Bowman's element and of the intermediate

segment causes them to return to their normal length after contraction, but does not lead to the appearance of the *proper clear segment*, which is only seen when the fibrils are forcibly extended to their full physiological length. The phenomena of contraction are essentially due to vital changes occurring in the tissue between the equator of Bowman's element and that of the intermediate segment.

Some Multinomial Theorems in Quaternions. By the Rev. M. M. U. Wilkinson. *Communicated by* Professor Tait.

(Read April 7, 1890.)

The formulæ established in a preceding paper are particular instances of some very general formulæ to which I wish to draw the attention of mathematicians.

A. *On Notation and Symbols employed.*

Suppose $(r+s)$ vectors taken, a_1, a_2, . . . a_{r+s}, and from these any selection of r vectors made. Calling the product of these vectors taken in order (that is, so that two vectors a_m, a_{m+n}, never occur in the order $a_{m+n}a_m$, but always in the order $a_m a_{m+n}$), p, and the product of the remaining s vectors, likewise taken in order, q, we form the four quaternions,

$$\mathrm{S}p\mathrm{S}q, \qquad \mathrm{S}.p\mathrm{V}q, \qquad \mathrm{V}p\mathrm{S}q, \qquad \mathrm{V}.p\mathrm{V}q,$$

if the sum of the suffixes in p is congruent to $\frac{1}{2}r(r+1)$, modulus 2 (that is to say, if the sum is odd or even according as the sum in what will be always regarded as the first term in the series is odd or even), the sign of the quaternion will be $+$, if otherwise, $-$. Four series are thus formed, each series containing $\mathrm{C}(r, s)$ terms, $\mathrm{C}(r, s)$ standing for the number of combinations of $(r+s)$ things taken r together. We define, as follows,

$$\mathrm{S}(r, s) = \Sigma \pm \mathrm{S}p\mathrm{S}q\,;$$

$$\mathrm{Z}(r, s) = \Sigma \pm \mathrm{S}.p\mathrm{V}q\,;$$

$$\mathrm{V}(r, s) = \Sigma \pm \mathrm{V}.p\mathrm{V}q\,;$$

$$\mathrm{B}(r, s) = \Sigma \pm \mathrm{V}.p\mathrm{S}q\,.$$

Thus, selecting out of $C(r, s)$ terms a number quite sufficient to indicate how all the other terms are formed,

$$\begin{aligned}V(r,s) = {} & V.\,a_1a_2\ldots a_r V a_{r+1}a_{r+2}\ldots a_{r+s} - V.\,a_1a_2\ldots a_{r-1}a_{r+1} V a_r a_{r+2}\ldots a_{r+s} + \ldots \\ & + V.\,a_1a_2\ldots a_{r-2}a_r a_{r+1} V a_{r-1}a_{r+2}\ldots a_{r+s} - \ldots + (-1)^{rs} V.\,a_{s+1}a_{s+2}\ldots a_{r+s} V a_1 a_2 \ldots a_s\,;\end{aligned}$$

Changing, in each quaternion, the second V into S, we get the series for $B(r, s)$: changing, in each quaternion, the first V only into S, we get the series for $Z(r, s)$: changing throughout V into S, we get the series for $S(r, s)$.

In the investigation we have to use series formed in exactly the same way, but with changed vectors (as, for instance, a_1 being omitted). It will be found, practically, quite sufficient to indicate what is meant by writing S′, S″, or S_ξ, &c., instead of S., &c. I have found it convenient that ξ shall represent Va_1a_2, and x the quaternion $a_3a_4\ldots a_{r+s}$, *i.e.*, the product of all the quaternions except the first two.

Of course S and Z are always scalars, and V and B are always vectors.

B. *Statement of Theorem.*

$$\left.\begin{aligned} S(2r, 2s) &= C(r, s)Sa_1a_2x\,; \\ Z(2r, 2s) &= 0\,; \\ V(2r, 2s) &= C(r, s-1)V.\,a_1a_2x\,; \\ B(2r, 2s) &= C(r-1, s)V.\,a_1a_2x\,; \end{aligned}\right\} \quad \ldots\ldots \quad (1)$$

$$\left.\begin{aligned} S(2r+1, 2s-1) &= 0\,; \\ Z(2r+1, 2s-1) &= 0\,; \\ V(2r+1, 2s-1) &= 2C(r, s-1)V.\,a_1a_2x\,; \\ B(2r+1, 2s-1) &= 0\,; \end{aligned}\right\} \quad \ldots\ldots \quad (2)$$

$$\left.\begin{aligned} S(2r, 2s+1) &= C(r, s-1)Sa_1a_2x\,; \\ Z(2r, 2s+1) &= 3C(r-1, s)Sa_1a_2x\,; \\ V(2r, 2s+1) &= C(r, s)Va_1a_2x\,; \\ B(2r, 2s+1) &= 0\,; \end{aligned}\right\} \quad \ldots\ldots \quad (3)$$

$$\left.\begin{aligned} S(2r+1, 2s) &= C(r-1, s)Sa_1a_2x\,; \\ Z(2r+1, 2s) &= 3C(r, s-1)Sa_1a_2x\,; \\ V(2r+1, 2s) &= 0\,; \\ B(2r+1, 2s) &= C(r, s)Va_1a_2x. \end{aligned}\right\} \quad \ldots\ldots \quad (4)$$

C. *Fundamental Cases.*

By fundamental cases we mean cases in which either one of the numbers r, s, in (1), (2), (3), (4) is unity. Some of these are the cases considered in my former paper, which proved that,

$$\begin{aligned} B(1, 2n+1) &= 0\,; \\ B(1, 2n) &= Va_1a_2x\,; \\ B(2, 2n) &= Va_1a_2x\,; \\ B(2, 2n+1) &= 0\,; \end{aligned}$$

of course we have, identically, and at once,

$$S(1, r) = S(r, 1) = B(r, 1) = 0\,;$$

and my former paper also showed that,

$$\begin{aligned} S(2, 2n) &= (n+1)Sa_1a_2x\,; \\ S(2, 2n+1) &= nSa_1a_2x\,; \end{aligned}$$

and so in other cases, and for Vs and Zs. But as we shall establish our formulæ (1) to (4) by an induction, it will be well to put down the following simple identities:—

$$S(1, 1) = 0\,;\; Z(1, 1) = 0\,;\; V(1, 1) = 2Va_1a_2\,;\; B(1, 1) = 0\,;$$

$$S(1, 2) = 0\,;\; Z(1, 2) = 3Sa_1a_2a_3\,;\; V(1, 2) = 0\,;\; B(1, 2) = V.\,a_1a_2a_3\,;$$

$$S(2, 1) = 0\,;\; Z(2, 1) = 3Sa_1a_2a_3\,;\; V(2, 1) = Va_1a_2a_3\,;\; B(2, 1) = 0\,;$$

$$S(1, 3) = 0\,;\; Z(1, 3) = 0\,;\; V(1, 3) = 2Va_1a_2a_3a_4\,;\; B(1, 3) = 0\,;$$

$$S(2, 2) = 2Sa_1a_2a_3a_4\,;\; Z(2, 2) = 0\,;\; V(2, 2) = Va_1a_2a_3a_4\,;\; B(2, 2) = Va_1a_2a_3a_4\,;$$

$$S(3, 1) = 0\,;\; Z(3, 1) = 0\,;\; V(3, 1) = 2Va_1a_2a_3a_4\,;\; B(3, 1) = 0.$$

No difficulty whatever presents itself in any one of these cases. So we leave them for the beginner. All others have, doubtless, in some form or other, met with them frequently before.

So, too, we leave such formulæ as,

$$\begin{aligned} Z(1, 2r) &= 3S(3,\; 2r-2)\,; \\ V(2r+1, 1) = V(1, 2r+1) &= 2V.\,a_1a_2x\,; \\ V(2r, 1) &= V.\,a_1a_2x. \end{aligned}$$

$$\begin{aligned} Z(2, 2n+1) &= 3S(3,\; 2n)\,; \\ Z(2, 2n) &= 0. \end{aligned}$$

$$\begin{aligned} V(2, 2n) = B(2n, 2) &= nVa_1a_2x\,; \\ V(2, 2n+1) &= (n+1)Va_1a_2x\,;\;\; \&c.\; \&c. \end{aligned}$$

They will be found to be formulæ really established by the investigation to which we now proceed.

D. *General Investigation.*

We assume that (1) to (4), which have been established in fundamental cases, hold when the total number of vectors $(m+n)$ involved does not exceed some odd numbers $(2\sigma+1)$. First, we will show that they still hold when $(m+n)$ does not exceed $(2\sigma+2)$: next when it does not exceed $(2\sigma+3)$. That is to say, assuming formulæ (1) to (4) as they are, we have first to find $S(2r+2, 2s)$, $S(2r, 2s+2)$, $S(2r+1, 2s+1)$, &c., and then $S(2r+1, 2s+2)$, $S(2r+2, 2s+1)$, &c. If the same law is found to hold, the formulæ will have been completely established.

We have,

$$\begin{aligned} S(2r, 2s+2) &= S.\alpha_1 B'(2r-1, 2s+2) + S.\alpha_1 B'(2s+1, 2r) \\ &= \{C(r-1, s+1) + C(s,r)\} S\alpha_1\alpha_2 x \\ &= C(r, s+1) S\alpha_1\alpha_2 x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5) \end{aligned}$$

So too, $$S(2r+2, 2s) = S(2s, 2r+2) = C(s, r+1) S\alpha_1\alpha_2 x \, ; \quad . \quad . \quad (6)$$

$$Z(2r, 2s+2) = S.\alpha_1 V'(2r-1, 2s+2) + S.\alpha_1 V'(2s+1, 2r) \, ;$$

or, $$Z(2r, 2s+2) = 0 \, ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

so too, $$Z(2r+2, 2s) = 0 \, ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$S(2r+1, 2s+1) = S.\alpha_1 B'(2r, 2s+1) - S\alpha_1 B'(2s, 2r+1) \, ;$$

or, $$S(2r+1, 2s+1) = 0 \, ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$\begin{aligned} Z(2r+1, 2s+1) &= S\alpha_1 V'(2r, 2s+1) - S\alpha_{2s+2} \ldots \alpha_{2r+2s+2} V\alpha_1\alpha_2 \ldots \alpha_{2s+1} + \ldots \\ &= S\alpha_1 V'(2r, 2s+1) - S.\alpha_1\alpha_2 \ldots \alpha_{2s+1} V\alpha_{2s+2} \ldots + \ldots \\ &= S.\alpha_1 V'(2r, 2s+1) - S.\alpha_1 V'(2s, 2r+1) \\ &= \{C(r, s) - C(s, r)\} S\alpha_1\alpha_2 x \, ; \end{aligned}$$

or, $$Z(2r+1, 2s+1) = 0 \, ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

assume,

$$V(2r, 2s+2) = V_1 + V_2 + V_3 + V_4 \, ,$$

where, in

V_1, a_1, a_2, both occur in first quaternion in each term ;

" V_4 " " last " "

" V_2, a_1 occurs in the first, and a_2 in the last quaternion ;

" V_3, a_2 " " a_1 " "

Then,

$$V_1 + V_2 = V.a_1\{Z'(2r-1, 2s+2) + V'(2r-1, 2s+2)\}$$
$$= 3C(r-1, s)a_1 Sa_2 x ;$$
$$V_1 = V.a_1 a_2\{Z''(2r-2, 2s+2) + V''(2r-2, 2s+2)\}$$
$$= C(r-1, s)V.a_1 a_2 Vx ;$$
$$V_3 = -3C(r-1, s)a_2 Sa_1 x + C(r-1, s)V.a_2 a_1 Vx ;$$
$$V_4 = V.a_3 a_4 \ldots a_{2r+2} Va_1 a_2 a_{2r+3} \ldots a_{2r+2s+2} - \ldots$$
$$= V''(2r, 2s)Sa_1 a_2 + V.a_3 a_4 \ldots a_{2r+2} V\xi a_{2r+3} \ldots a_{2r+2s+2} - \ldots$$
$$V_\xi(2r, 2s+1) = V.\xi a_3 \ldots a_{2r+1} Va_{2r+2} \ldots - \ldots + V.a_3 a_4 \ldots a_{2r+2} V\xi a_{2r+3} \ldots - \ldots$$
$$= V.\xi\{Z''(2r-1, 2s+1) + V''(2r-1, 2s+1)\} +$$
$$V.a_3 a_4 \cdots a_{2r+2} V\xi a_{2r+3} \ldots - \ldots ;$$
$$V_4 = C(r, s-1)VxSa_1 a_2 + C(r, s)V\xi x - 2C(r-1, s)V.\xi Vx ;$$
$$V_1 + V_2 + V_3 + V_4 = C(r-1, s)\{3a_1 Sa_2 x - 3a_2 Sa_1 x + V.a_2 a_1 Vx - 2V.\xi Vx\} +$$
$$+ C(r, s)V\xi x + C(r, s-1)VxSa_1 a_2 =$$
$$= C(r-1, s)\{3V.a_1 a_2 Vx - 3VxSa_1 a_2 + VxSa_1 a_2 - 3V.\xi Vx\}$$
$$+ C(r, s)V\xi x + C(r, s-1)VxSa_1 a_2, \quad \text{or}$$
$$V(2r, 2s+2) = C(r, s)Va_1 a_2 x ; \qquad . \qquad . \qquad (11)$$

In our formulæ we may, of course, write $r+1$ for r and $s-1$ for s, and thus obtain in the same way,

$$V(2r+2, 2s) = C(r+1, s-1)Va_1 a_2 x ; \quad . \qquad . \quad (12)$$

Again, if

$$B(2r+2, 2s) = B_1 + B_2 + B_3 + B_4 ,$$
$$B_1 + B_2 = V.a_1\{S'(2r+1, 2s) + B'(2r+1, 2s)\}$$
$$= C(r-1, s)a_1 Sa_2 x + C(r, s)V.a_1 Va_2 x ;$$
$$B_1 = V.a_1 a_2\{S''(2r, 2s) + B''(2r, 2s)\}$$
$$= C(r, s)\xi Sx + C(r-1, s)V.\xi Vx ;$$

$$B_3 = -C(r-1, s)a_2Sa_1x - C(r, s)V.a_2Va_1x + C(r, s)Va_2a_1Sx$$
$$+ C(r-1, s)V.a_2a_1Vx$$
$$= C(r-1, s)(VxSa_1a_2 - a_1Sa_2x) + C(r, s)(a_2Sa_1x - V.a_2a_1Vx) ;$$
$$B_4 = Va_3a_4 \ldots a_{2r+4}Sa_1a_2a_{2r+5} \cdots - \cdots$$
$$= B''(2r+2, 2s-2)Sa_1a_2 + Va_3a_4 \ldots a_{2r+4}S\xi a_{2r+5} \cdots - \cdots$$
$$B_\xi(2r+2, 2s-1) = V.\xi a_3a_4 \ldots a_{2r+3}Sa_{2r+4} \cdots - \cdots +$$
$$V.a_3a_4 \ldots a_{2r+4}S\xi a_{2r+5} \cdots - \cdots$$
$$O = V.\xi\{S''(2r+1, 2s-1) + Z''(2r+1, 2s-1)\} +$$
$$V.a_3a_4 \ldots a_{2r+4}S\xi a_{2r+5} \cdots - \cdots$$
$$B_4 = C(r, s-1)VxSa_1a_2 ;$$
$$B_1 + B_2 + B_3 + B_4 = C(r-1, s)VxSa_1a_2 + C(r, s-1)VxSa_1a_2 +$$
$$C(r, s)(Va_1a_2x - VxSa_1a_2) ;$$
$$B(2r+2, 2s) = C(r, s)Va_1a_2x ; \quad . \quad . \quad . \quad . \quad (13)$$

So too, $\quad B(2r, 2s+2) = C(r-1, s+1)Va_1a_2x ; \quad . \quad . \quad . \quad (14)$

Proceeding in the same way, take

$$V(2r+1, 2s+1) = V_1 + V_2 + V_3 + V_4 ;$$
$$V_1 + V_2 = V.a_1\{Z'(2r, 2s+1) + V'(2r, 2s+1)\}$$
$$= 3C(r-1, s)a_1Sa_2x + C(r, s)V.a_1Va_2x$$
$$V_1 = V.a_1a_2\{Z''(2r-1, 2s+1) + V''(2r-1, 2s+1\}$$
$$= 2C(r-1, s)V.a_1a_2Vx ;$$
$$V_3 = -3C(r-1, s)a_2Sa_1x - C(r, s)V.a_2Va_1x + 2C(r-1, s)V.a_2a_1Vx ;$$
$$V_4 = V.a_3a_4 \ldots a_{2r+3}Va_1a_2a_{2r+4} \ldots - \ldots .$$
$$= V''(2r+1, 2s-1)Sa_1a_2 + V.a_3a_4 \ldots . a_{2r+3}V\xi a_{2r+4} \ldots . - ..$$
$$V_\xi(2r+1, 2s) = V.\xi a_3a_4 \ldots a_{2r+2}Va_{2r+3} \ldots - \ldots - V.a_3a_4 \ldots a_{2r+3}V\xi a_{2r+4} \ldots + ..$$
$$O = V.\xi\{Z''(2r, 2s) + V''(2r, 2s)\} - V.a_3a_4 \ldots a_{2r+3}V\xi a_{2r+4} \ldots . + \ldots$$
$$V_4 = 2C(r, s-1)VxSa_1a_2 + C(r, s-1)V.\xi Vx ;$$
$$V_1 + V_2 + V_3 + V_4 = C(r-1, s)(3a_1Sa_2x - 3a_2Sa_1x + 2Va_2a_1Vx) +$$
$$+ C(r, s)(V.a_1Va_2x - V.a_2Va_1x) + C(r, s-1)(2VxSa_1a_2 + V.\xi V\hat{x})$$
$$= \{C(r-1, s) + C(r, s-1)\}(a_1Sa_2x - a_2Sa_1x + 2VxSa_1a_2) +$$
$$+ C(r, s)(V.a_1a_2x - a_1Sa_2x + a_2Sa_1x - Va_2a_1x) .$$

So that

$$V(2r+1,\ 2s+1)=2C(r,\ s)V.\alpha_1\alpha_2x\ ; \quad . \quad . \quad . \quad (15)$$

So also, if

$$B(2r+1,\ 2s+1)=B_1+B_2+B_3+B_4\,,$$

$$B_1+B_2=V.\alpha_1\{S'(2r,\ 2s+1)+B'(2r,\ 2s+1)\}=C(s-1,\ r)\alpha_1S\alpha_2x\ ;$$

$$B_1=V.\alpha_1\alpha_2\{S''(2r-1,\ 2s+1)+B''(2r-1,\ 2s+1)\}=0\ ;$$

$$B_3=-C(s-1,\ r)\alpha_2S\alpha_1x\ ;$$

$$B_4=V.\alpha_3\alpha_4\ldots\alpha_{2r+3}S\alpha_1\alpha_2\alpha_{2r+4}\ldots-\ldots$$

$$=B''(2r+1,\ 2s-1)S\alpha_1\alpha_2+V.\alpha_3\alpha_4\ldots\alpha_{2r+3}S\xi\alpha_{2r+4}\ldots-\ldots\ ;$$

$$B_\xi(2r+1,\ 2s)=V.\xi\alpha_3\alpha_4\ldots\alpha_{2r+2}S\alpha_{2r+3}\ldots-\ldots-V.\alpha_3\alpha_4\ldots\alpha_{2r+3}S\xi\alpha_{2r+4}\ldots+\ldots\ ;$$

$$B_4=-C(r,\ s)V\xi x+V.\xi\{S''(2r,\ 2s)+B''(2r,\ 2s)\}$$

$$=-C(r,\ s)V\xi x+C(r,\ s)\xi Sx+C(r-1,\ s)V.\xi Vx$$

$$=-C(r,\ s-1)V.\xi Vx\ ;$$

whence, since $\quad V.\xi Vx=\alpha_1S\alpha_2x-\alpha_2S\alpha_1x\,,$

$$B(2r+1,\ 2s+1)=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Thus far we have shown that, if formulæ (1) to (4) are true for all number of vectors up to an odd number inclusive, they hold when the number of vectors is the next even number.

Proceeding to the next odd number of vectors, we have

$$S(2r+1,\ 2s+2)=S.\alpha_1B'(2r,\ 2s+2)+S.\alpha_1B'(2s+1,\ 2r+1)\,,$$

the sign of the second term being $+$. Hence,

$$S(2r+1,\ 2s+2)=C(r-1,\ s+1)S\alpha_1\alpha_2x\ ; \quad . \quad . \quad (17)$$

So too, $\quad S(2r+2,\ 2s+1)=C(r+1,\ s-1)S\alpha_1\alpha_2x\ ; \quad . \quad . \quad (18)$

Again, $\quad Z(2r+1,\ 2s+2)=S\alpha_1V'(2r,\ 2s+2)+$

$$+S\alpha_{2s+3}\ldots\ldots\alpha_{2r+2s+3}V\alpha_1\alpha_2\ldots\alpha_{2s+2}-\ldots$$

$$=C(r,\ s)S\alpha_1\alpha_2x+S\alpha_1\alpha_2\ldots\alpha_{2s+2}V\alpha_{2s+3}\ldots-\ldots$$

$$=C(r,\ s)S\alpha_1\alpha_2x+S\alpha_1V'(2s+1,\ 2r+1)$$

$$=3C(r,\ s)S\alpha_1\alpha_2x\ ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

So too, $\quad Z(2r+2,\ 2s+1)=3C(r,\ s)S\alpha_1\alpha_2x\ ; \quad . \quad . \quad . \quad . \quad (20)$

And, proceeding on the same lines as before,

if $\quad V(2r+1,\ 2s+2)=V_1+V_2+V_3+V_4\,,$

$$V_1 + V_2 = V.a_1\{Z'(2r, 2s+2) + V'(2r, 2s+2)\}$$
$$= C(r, s)V.a_1Va_2x ;$$
$$V_1 = V.a_1a_2\{Z'(2r-1, 2s+2) + V'(2r-1, 2s+2)\}$$
$$= 3C(r-1, s)Va_1a_2Sx ;$$
$$V_3 = -C(r, s)V.a_2Va_1x + 3C(r-1, s)Va_2a_1Sx ;$$
$$V_4 = V.a_3a_4 \dots a_{2r+3}Va_1a_2a_{2r+4} \dots - \dots$$
$$= V.''(2r-1, 2s+2)Sa_1a_2 + V.a_3a_4 \dots a_{2r+3}V\xi a_{2r+4} \dots - \dots$$
$$V_\xi(2r+1, 2s+1) = V.\xi a_3a_4 \dots a_{2r+2}Va_{2r+3} \dots - \dots - V.a_3a_4 \dots a_{2r+3}V\xi a_{2r+4} \dots + .$$
$$V_4 = -2C(r, s)V\xi x + V.\xi\{Z''(2r, 2s+1) + V''(2r, 2s+1)\}$$
$$= -2C(r, s)V\xi x + 3C(r-1, s)\xi Sx + C(r, s)V.\xi Vx ;$$
$$V_1 + V_2 + V_3 + V_4 = C(r, s)(V.a_1Va_2x - V.a_2Va_1x - 2V\xi x + V.\xi Vx) +$$
$$+ 3C(r-1, s)(Va_2a_1Sx + \xi Sx) =$$
$$= C(r, s)(V.a_1a_2x - V.a_2a_1x - 2V\xi x) = 0 ;$$

or,
$$V(2r+1, 2s+2) = 0 ; \quad . \quad . \quad . \quad . \quad . \quad (21)$$

Again, if
$$V(2r+2, 2s+1) = V_1 + V_2 + V_3 + V_4 ,$$
$$V_1 + V_2 = V.a_1\{Z'(2r+1, 2s+1) + V'(2r+1, 2s+1)\}$$
$$= 2C(r, s)V.a_1Va_2x ;$$
$$V_1 = V.a_1a_2\{Z''(2r, 2s+1) + V''(2r, 2s+1)\}$$
$$= 3C(r-1, s)V.a_1a_2Sx + C(r, s)V.a_1a_2Vx ;$$
$$V_3 = -2C(r, s)V.a_2Va_1x + 3C(r-1, s)V.a_2a_1Sx + C(r, s)V.a_2a_1Vx ;$$
$$V_4 = V.a_3a_4 \dots a_{2r+4}Va_1a_2a_{2r+5} \dots - \dots$$
$$= V''(2r+2, 2s-1)Sa_1a_2 + V.a_3a_4 \dots a_{2r+4}V\xi a_{2r+5} \dots - \dots$$
$$V_\xi(2r+2, 2s) = V.\xi a_3a_4 \dots a_{2r+3}Va_{2r+4} \dots - \dots + V.a_3a_4 \dots a_{2r+4}V\xi a_{2r+5} \dots - \dots$$

So that,
$$V_4 = C(r+1, s-1)VxSa_1a_2 + C(r+1, s-1)V\xi x$$
$$- V.\xi\{Z''(2r+1, 2s) + V''(2r+1, 2s)\}$$
$$= C(r+1, s-1)Va_1a_2x - 3C(r, s-1)\xi Sx ;$$
$$V_1 + V_2 + V_3 + V_4 = C(r, s)(2V.a_1Va_2x - 2V.a_2Va_1x + V.a_2a_1Vx) +$$
$$+ 3C(r-1, s)V.a_2a_1Sx - 3C(r, s-1)\xi Sx + C(r+1, s-1)Va_1a_2x$$
$$= C(r, s)(2Va_1a_2x - 2Va_2a_1x - 2V.\xi Vx + V.a_2a_1Vx - 3\xi Sx) +$$
$$+ C(r+1, s-1)Va_1a_2x$$

$$= \mathrm{C}(r, s)(4\mathrm{V}\xi x - 2\mathrm{V}\xi x + x\mathrm{S}a_1a_2 - \mathrm{V}\xi\mathrm{V}x - \xi\mathrm{S}x)$$
$$+ \mathrm{C}(r+1, s-1)\mathrm{V}a_1a_2x$$
$$= \mathrm{C}(r, s)\mathrm{V}a_1a_2x + \mathrm{C}(r+1, s-1)\mathrm{V}a_1a_2x\,;$$

So that $$\mathrm{V}(2r+2, 2s+1) = \mathrm{C}(r+1, s)\mathrm{V}a_1a_2x\,; \quad . \quad . \quad . \qquad (22)$$

Again, $$\mathrm{B}(2r+2, 2s+1) = \mathrm{B}_1 + \mathrm{B}_2 + \mathrm{B}_3 + \mathrm{B}_4\,, \text{ where,}$$

$$\mathrm{B}_1 + \mathrm{B}_2 = \mathrm{V}.\,a_1\{\mathrm{S}'(2r+1, 2s+1) + \mathrm{B}'(2r+1, 2s+1)\} = 0\,;$$
$$\mathrm{B}_1 = \mathrm{V}.\,a_1a_2\{\mathrm{S}''(2r, 2s+1) + \mathrm{B}''(2r, 2s+1)\}$$
$$= \mathrm{C}(r, s-1)\xi\mathrm{S}x\,;$$
$$\mathrm{B}_3 = \mathrm{C}(r, s-1)\mathrm{V}a_2a_1\mathrm{S}x\,;$$
$$\mathrm{B}_4 = \mathrm{V}.\,a_3a_4\ldots a_{2r+4}\mathrm{S}a_1a_2a_{2r+5}\ldots - \ldots$$
$$= \mathrm{B}''(2r+2, 2s-1)\mathrm{S}a_1a_2 + \mathrm{V}.\,a_3a_4\ldots a_{2r+4}\mathrm{S}\xi a_{2r+5}\ldots\ldots - \ldots$$
$$\mathrm{B}_\xi(2r+2, 2s) = \mathrm{V}.\,\xi a_3a_4\ldots a_{2r+3}\mathrm{S}a_{2r+4}\ldots - \ldots + \mathrm{V}.\,a_3a_4\ldots a_{2r+4}\mathrm{S}\xi a_{2r+5}\ldots - \ldots$$
$$\mathrm{B}_4 = \mathrm{C}(r, s)\mathrm{V}\xi x - \mathrm{V}.\,\xi\{\mathrm{S}''(2r+1, 2s) + \mathrm{B}''(2r+1, 2s)\}$$
$$= \mathrm{C}(r, s)\mathrm{V}\xi x - \mathrm{C}(r-1, s)\xi\mathrm{S}x - \mathrm{C}(r, s)\mathrm{V}.\,\xi\mathrm{V}s\,;$$
$$\mathrm{B}_1 + \mathrm{B}_2 + \mathrm{B}_3 + \mathrm{B}_4 = \mathrm{C}(r, s)(\mathrm{V}\xi x - \xi\mathrm{S}x - \mathrm{V}.\,\xi\mathrm{V}x) = 0\,;$$

or, $$\mathrm{B}(2r+2, 2s+1) = 0\,; \quad . \quad . \quad . \quad . \quad . \qquad (23)$$

And we can easily show that

$$\mathrm{V}(2r+2, 2s+1) - \mathrm{B}(2r+2, 2s+1) = \mathrm{B}(2s+1, 2r+2) - \mathrm{V}(2s+1, 2r+2)\,;$$

so that

$$\mathrm{B}(2s+1, 2r+2) = \mathrm{V}(2r+2, 2s+1) + \mathrm{V}(2s+1, 2r+2)$$
$$= \mathrm{C}(r+1, s)\mathrm{V}a_1a_2x\,; \quad . \quad . \quad . \quad . \quad . \quad . \qquad (24)$$

So that our formulæ (1) to (4) are completely established.

On an Accidental Illustration of the Effective Ohmic Resistance to a Transient Electric Current through a Steel Bar. By Sir William Thomson.

(Read March 17, 1890.)

After the recent meeting of the British Association at Newcastle, Lord Armstrong, in showing me the appliances by which his house at Cragside is lighted electrically by water-power, told me of a very wonderful incident which he had recently experienced. A bar of steel, which he was holding in his hand, was allowed accidentally to come in contact with the two poles of a dynamo in action. He

instantly perceived a painful sensation of burning, and let the bar drop. He found his hand or fingers, where it had touched the bar, severely blistered. The bar itself was found immediately afterwards to be quite cold, or not perceptibly hot. This was a very marvellous incident. It proved (1) the outer surface of the steel to have been intensely heated; (2) that not enough of heat was generated to sensibly warm the whole bar. The explanation, of course, was to be found in the known laws of diffusion of electric currents, through non-magnetic conductors, considered in connection with the effect of magnetic susceptibility of unknown amount and law, in conductors of steel or iron.

Lord Armstrong's accidental experiment seemed to me such a very instructive illustration of fundamental principles of electromagnetic induction, that I wrote to him asking his permission to communicate it to the Royal Society of Edinburgh, at the same time inquiring as to some details. In reply I immediately received a letter, of date 7th March, kindly giving the desired permission, and containing the following very interesting statement:—

"I send you, by parcel post, the steel (not iron) bar which I held " when it accidentally short-circuited the current. You will observe " two little hollows,* which were burnt out of the metal at the " instant of contact, and these mark the distance between the points " of contact. The bar was held by my fingers midway between " these two marks, and the burns were inflicted at the places where " my fingers touched the metal. The sudden pain caused me to " dash the bar instantaneously to the ground, and an attendant " *immediately* picked it up and found it quite cold. Three of my " fingers and my thumb were blistered, and had the injuries not " been immediately treated by an expert who happened to be " present, they would probably have developed into troublesome " sores; as it was, my arm had to be carried in a sling during the " first day, and I was not able to hold a pen with comfort for many " days afterwards. There was a great blaze of light from the two " points of metallic contact, but the flame could not possibly have " got under my fingers where they touched the metal and were " burnt. If the flame had done the injury, it would have taken

* The distance between the hollows is 15½ cms., the bar is about a foot long, and its diameter is 14 mm.

"effect upon the exposed parts of my hand, but nothing was scorched "except the skin at the points of grasp.

"The dynamo was of Crompton's pattern, with compound wind- "ing. The speed was about 1300 revolutions per minute. The "dynamo was not employed in charging batteries, but in the direct "lighting of incandescent lamps. The duty of the dynamo at the "time would be about 85 amperes and the potential 103 volts. No "check in the dynamo was perceived, nor was it likely to be observ- "able, seeing that the momentum of the revolving parts would be "enormously powerful to overcome any momentary increase of re- "sistance. There being two dynamos on one axis, both in motion, "though only one was doing work, besides the turbine-wheel and "the impinging jets, there would be a collective momentum of great "energy for a momentary effort."

The requisites for working out fully the theory of transient or periodic currents in conductors of any form, are included in Maxwell's fundamental equations of electro-magnetic induction, and are given explicitly for straight cylindric conductors of non-magnetic material in §§ 685–689 of his great work. Lord Rayleigh in his paper "On the Self-Induction and Resistance of Straight Conductors" (*Phil. Mag.*, May 1886) gives explicitly the proper formulas for transient or periodic currents, in straight cylindric rods of iron, on the supposition of constant magnetic susceptibility. The details of this highly interesting and important branch of the subject have also been investigated by Heaviside in a very comprehensive manner. The tendency of periodically alternating currents, to be condensed in the outer part of a cylindric conductor, while the current may be exceedingly feeble or quite insensible in the central parts, was discussed and explained by Lord Rayleigh in p. 388 of his article already referred to, and its aggravation in an iron conductor specially pointed out. The same considerations show that a transient current resulting from the application, for a very short time, of the electro-motive force of a voltaic battery, or of electro magnetic induction acting not directly on * the cylindric conductor

* This caution is introduced to avoid leading any reader into an error into which I myself fell in the text, and corrected in footnotes, of an article in *Philosophical Magazine* for March 1890, "On the Time-Integral of a Transient Electro-Magnetically Induced Current."

considered, but on a conductor such as the inductor of a dynamo of any kind, momentarily in circuit with it, is only skin deep if the duration of the electromotive force be but short enough; and that the depth to which the current penetrates in a given very short time is much smaller for iron than for copper. This is certainly the explanation of Lord Armstrong's wonderful experiment.

To find something towards a mathematical solution for the increasing current at any instant during the electric contact of Lord Armstrong's experiment, it is convenient first to solve the problem of finding the subsidence of current initially given in a circuit of two very long parallel bars connected by end bridges, or in a circuit of one long bar insulated within a conducting sheath except at its ends, which are in metallic connection with the sheath.

In all cases of electric currents given in parallel straight lines, and left to subside,* without any other electromotive force than that of their mutual electro-magnetic induction, the thermal analogy is exceedingly convenient. For electric conductivity, c, we have thermal capacity divided by 4π; for magnetic permeability, the reciprocal of thermal conductivity; and for current-density, temperature multiplied by thermal capacity. Thus, if two infinitely long straight parallel conducting bars, separated by insulating material, be given with equal currents in opposite directions through them, and left to themselves, we have precisely the same mathematical problem to solve as if in every line of the thermal analogue, we had

* The thermal analogue for a varying or constant electromotive force applied by a voltaic battery or dynamo, substituted for one of the end-bridges, is positive and negative sources of heat applied at the interfaces between the thermal analogues of electric conductor and electric insulator. The quantity of heat generated per unit of area per unit of time, at any point of either interface in the thermal analogue, is equal to the rate of variation per unit of length along the electric conductor, of the electrostatic force in the insulator in contact with it in the electric analogue. Remark that, in the electric system the potential is uniform over each normal section of either conductor, and that the variation of potential within each conductor per unit of distance along its length—that is to say, the component electrostatic force in the direction of the length is exceedingly small in comparison with the component electrostatic force perpendicular to the length at any place in the insulator, except close to the ends metallically connected by a bridge. The equations in the text are unchanged, except the second interfacial condition, it becomes

$$\sigma + \frac{1}{\varpi}\left[\frac{d\zeta}{d\nu}\right] - \frac{1}{\varpi'}\left[\frac{d\zeta}{d\nu}\right]',$$

where σ denotes the quantity of heat generated per unit of time in the source.

initial temperature multiplied by thermal capacity given equal to the current-density in the corresponding line of the electro-magnetic problem, and the system left to itself, with the positive and negative temperatures in the two bars subsiding towards zero.

The thermal analogue for the insulating material of the electro-magnetic problem is an ideal medium of zero thermal capacity. Thus in process of equalisation of temperature we have diffusion of heat through the substance of each bar, according to Fourier's original use of the term diffusion; while in the ideal medium taking the place of the electric insulator, we have merely conduction of heat, without any diffusion properly so called, that is to say, without any excess of heat conducted out of, above heat conducted into, any portion of the medium.

If ζ denote the temperature at time t, in the thermal analogue, at any point, P; $4\pi c$ the thermal capacity per unit of volume; $1/\varpi$ the thermal conductivity of the analogue to either of the electric conducting bars; and $1/\varpi'$ the thermal conductivity of the analogue to the insulating medium: the equations expressing all the conditions of the problem are

$$4\pi c\frac{d\zeta}{dt}=\frac{1}{\varpi}\left(\frac{d^2\zeta}{dx^2}+\frac{d^2\zeta}{dy^2}\right),\ \text{[P, in either bar]}$$

$$0=\frac{d^2\zeta}{dx^2}+\frac{d^2\zeta}{dy^2},\ \text{[P, in the analogue to the insulating medium]}:$$

$$\left.\begin{aligned}[\zeta]&=[\zeta]'\\ \frac{1}{\varpi}\left[\frac{d\zeta}{dv}\right]&=\frac{1}{\varpi'}\left[\frac{d\zeta}{dv}\right]'\end{aligned}\right\}\text{[P, in the interface]};$$

where d/dv denotes rate of variation per unit length in the direction of the normal, at any point of the interface; and [ ], and [ ]′ denote values infinitely near the interface outside and inside respectively.

In the particular case of one of the bars of circular cross-section, and the other a hollow circular cylinder surrounding it coaxially, the problem becomes greatly simplified. It becomes still farther so if we suppose the electric conductivity, or in the thermal analogue the thermal capacity, of this outside sheath to be infinitely great. In this last case we have identically the same mathematical problem

as that regarding a heated cylinder left to cool, which was presented and fully solved by Fourier in the sixth chapter of his great work (*Théorie analytique de la Chaleur*). Instead of the bodily thermal conductivity divided by surface emissivity of Fourier's problem, essentially a line which we shall denote by λ, we have in the electro-magnetic problem,

$$\lambda = \frac{\varpi'}{\varpi} a \log \frac{b}{a},$$

when ϖ and ϖ' are the magnetic permeabilities of the conducting rod and insulating medium around it, and a and b the radii of the cross-sections of the rod, and of the inner surface of the enclosing conductor.

Not considering for the present the interesting case suggested by Heaviside of an insulating medium composed of soft iron filings imbedded in wax or other ordinary insulating solid, we have practically $\varpi' = 1$, whether the insulator be air or any ordinary insulating solid or liquid.

Consider now two cases—a copper rod and a steel or iron rod, each of the same diameter, 1·4 cm. as Lord Armstrong's steel rod, and suppose, for example, b equal to ten times a, we have

$$\lambda = 2{\cdot}3a \doteqdot 1{\cdot}6 \text{ cm. for copper;}$$
$$\lambda = \varpi^{-1}.2{\cdot}3a = \varpi^{-1}.1{\cdot}6 \text{ cm. for steel or iron.}$$

If b, instead of being 7 cm. were 70 cm. or 700 cm., λ would be only doubled or tripled; on the other hand, if b were very small in comparison with a, $\lambda \doteqdot b - a$. Excluding this case, we see that for copper λ is greater than a, or not incomparably less than a. We thus have a very fine and a very easy example for working out numerical results by Fourier's solution.

On the other hand, for an iron or steel rod we have for ϖ some large number, possibly about 300; or if the currents and therefore the magnetic forces concerned are very small, we may, according to Lord Rayleigh's important experimental investigation on the subject of magnetic induction * by very small magnetic forces, have ϖ as

* This collocation of words illustrates the exceeding inconvenience of Maxwell's use of "magnetic induction" to designate the magnetic force in an air-crevasse perpendicular to the lines of magnetisation in magnetised steel or soft iron.

small as 80; on the other hand, for moderately strong currents and correspondingly high electromotive forces, we may have ϖ greater than 3000.* We shall take it as 300 merely by way of example and illustration, but as the permeability varies enormously with the amount of the magnetising force, and in a manner desperately complicated by magnetic retentiveness, hysteresis according to Ewing's designation, no accurate mathematical investigation is practicable with only our present knowledge of the requisite data for the diffusion of electric currents through an iron or steel conductor.

Taking for iron or steel $\varpi = 300$, and, as above, $a = \cdot 7$, $b = 7$, we find $\lambda = 1/130$ of a, or $1/187$ of a centimetre. Now, because λ is so small a fraction as 1/130 of the radius of the rod, we see that the current-density at the surface (or surface-temperature in the thermal analogue) drops nearly to zero, while there is still but a relatively small diminution of current-density (or temperature in the thermal analogue) farther in from the surface than a distance of 1/10 of the radius. Hence, for the roughly approximate investigation with which we must be content, we may be satisfied with the very simplifying supposition of $\lambda = 0$.

If we take 10,000 c.g.s. (or square centimetres per second) as the resistivity † of steel or iron, we must divide this by $300 \times 4\pi$ to find the diffusivity for electric current (thermal diffusivity, or conductivity divided by thermal capacity of unit volume, in the thermal analogue), which therefore is 2·7 square centimetres per second. This is only about 12 times the thermal diffusivity of heat in iron (which is ·225 of a square centimetre per second). Hence in 1/4300 of a second (if $\lambda = 0$), the state of things as regards falling off of the strength of current towards the final zero, at different distances from the surface, would be that represented by number 0·1 curve of my diagram of laminar diffusion.‡ That is to say, the diffusion curve would be curve number 1 with its

* Rowland for one specimen of iron found the magnetic susceptibility as high as 3595 for magnetising force 1·317 (see *Phil. Mag.*, Aug. 1873).

† This seems to me a much better word than specific resistance to denote the resistance per centimetre of length of a bar of a square centimetre of cross-section of any substance. The resistivity of Lord Armstrong's steel bar I have found by measurement to be 14,000 c.g.s.

‡ See *British Association Report*, Bath, 1888, p. 571; or Vol. III. of my collected Papers, to be published in May.

vertical ordinates reduced to 1/10, or curve number 10 with its vertical ordinates reduced to 1/100. Now by number 1 curve we see that at 1/2 centimetre from the initiational surface (supposed plane) the amount of the falling off is 16 per cent. of the whole. Hence in our iron or steel rod (on the supposition $\lambda = 0$) the current at 1/4300 of a second from the beginning, and at 1/20 of a centimetre from the surface, would have fallen off by 16 per cent. of its given amount. Thus we judge that during the first 1/4000 of a second the effect of the cylindric curvature is but slight, and the diffusion follows sensibly the law of plane laminar diffusion.

The supposition of a circular cylindric sheath of infinite electric conductivity, coaxal with the rod considered, and separated from it by the insulating material, which we have adopted for the sake of simplicity and definiteness, may be departed from, and instead we may substitute any conductor parallel to the rod considered, provided that the distance between the two is a considerable multiple of the greatest diameter of either. In virtue of this proviso, the distribution of current-density is necessarily but very little disturbed from the equality at equal distances from the axis of the rod, which was provided for by the supposition of a cylindric sheath. If the second conductor is of the same diameter and of the same material as the first, and placed at a distance $2a$ from it, the expression for λ will be the same as that given above.

Now, instead of the great simplicity of a current, generated (no matter how), and given initially as a steady current, through the circuit of two long parallel conductors and the end-bridges between them; suppose the conductors and one end-bridge to be given with no current, and let a voltaic battery be suddenly applied instead of the other end-bridge. If the difference of potentials maintained by the latter between the ends to which it is applied be absolutely constant, the rise of the current through the parallel conductors from its initial zero to its ultimate steady amount will follow *nearly* the same law as the fall from the initial steady current to the final zero in our former simple case: *exactly* the same law if the quantity of positive electricity on one conductor, and of negative electricity on the other, called forth according to electro-static law, in virtue of the gradient of potential, is nothing in comparison with the quantity flowing through the circuit. The quantity of electricity required

for the static electrification is not negligible in a large variety of telegraphic and telephonic problems.* In the ocean cable problem it is of paramount importance; and the electro-magnetic induction with which we are now occupied is negligible. In shorter cables and high-speed action both the electro-static and electro-magnetic induction must be taken into account, and both have been very practically taken into account by Heaviside in the mathematical theory of the telephone.

In a vast variety of laboratory experimental arrangements the currents required for the static charges are quite negligible. In such a case as that of Lord Armstrong's experiment, the quantity of electricity required for producing or changing the electrification of every part of the circuits or of the conductors concerned is clearly quite insensible in comparison with the quantity which must have flowed through the bar to produce the observed heating effect.

But although we are not troubled with any difficulty in respect to electro-static charge, we have in Lord Armstrong's case circumstances of such extreme complexity that it is of no use to attempt to work out a complete mathematical theory. It seems probable, however, that the solution indicated above, and represented by my diffusion diagram, fairly illustrates the circumstances of the current which actually flowed through the steel bar, though scarcely with any approach to quantitative correctness. At all events we have a very striking illustration of what really took place, and ample explanation of the intensity and suddenness of the effect perceived by Lord Armstrong, by working out the result numerically of what would take place if a difference of potentials of 100 volts were suddenly instituted, and forcedly maintained constant during one or two or three ten thousandths of a second between two points of the bar $15\frac{1}{2}$ centimetres asunder. This with any reasonable assumption as to the magnetic permeability of the iron or steel bar, and its diffusivity for electric currents, is easily done on the supposition $\lambda = 0$, and the solution conveniently represented by the diffusion diagram.

We must not, however, suppose that the difference of potentials between the two points of the steel bar touched by the main electrodes of the dynamo was in reality constant at 103 volts, even for so long a time as two or three ten thousandths of a second. What was

* Papers, Vol. II. Arts. 72–77 and 80–83.

really constant may during a part of the time more really have been the strength of the current through the electrodes leading from the complex dynamo-circuit of armature and of shunt and series coils of the electro-magnet, to the external permanent electric lighting circuit and temporary circuit through the steel bar. The difference of potentials between the two points of the steel touched must have been at first 103 volts, and must have fallen very rapidly, while the current which it produced in the steel rose from 0 to 85 amperes, against ohmic resistance sinking from infinity towards ·000137 of an ohm (this being the actual resistance of Lord Armstrong's bar to a current running full-bore through it, as I have found by measurement).

The immense quasi-inertia of each partial circuit within the dynamo forbids the supposition that there can have been any great augmentation of the outgoing current during the few hundredths of a second of the short-circuiting by the steel bar; and, possibly with no practical error, we may suppose that current to have been constant during the whole time. Hence, at each instant the electric lighting circuit must have lost just as much current as that which was passing through the steel bar. Hence, considering the smallness of the quasi-inertia of the electric lighting circuit, the 85 amperes through it before the accident must, after two or three ten-thousandths of a second, have been very nearly annulled, and, therefore, very nearly a constant current of 85 amperes must have passed for the rest of the time through the outer skin of the steel bar.* We have thus

* This suggests an interesting and, happily, an easy problem regarding electro-magnetic induction in rectilinear electric current through a conductor surrounded by an insulator. Let the electromotive action, whatever its kind, be so regulated that the integral amount of current crossing the normal section of the conductor is kept constant. The mathematical statement of this condition, according to the notation of the text above, is,

$$\frac{d}{dt}\iint d\mathrm{A}\zeta = 0\,,$$

where $\iint d\mathrm{A}$ denotes surface integration over the cross-section of the conductor. From this, by the first of the equations of the text,

$$\iint d\mathrm{A}\left(\frac{d^2\zeta}{dx^2}+\frac{d^2\zeta}{dy^2}\right) = 0\,.$$

Now, as is well known, and very easily proved, we have in every case,

$$\iint d\mathrm{A}\left(\frac{d^2\zeta}{dx^2}+\frac{d^2\zeta}{dy^2}\right) = \int ds\,\frac{d\zeta}{d\nu}\,,$$

no difficulty in understanding that there should have been amply sufficient current through an exceedingly thin shell of the bar to produce very suddenly the high temperature of the surface which Lord Armstrong perceived, and yet that the total amount of heat generated was insufficient to heat the bar to any sensible degree after the second or two required for the thermal diffusion (diffusivity ·225 of a sq. cm. per sec.), to spread it nearly uniformly through the body of the bar. The heat lost outside the bar by surface emissivity (which is about 1/4000 of a gramme water thermal unit per second per sq. cm. of surface per degree of excess) would be quite ineffective to considerably diminish the whole quantity in the time required for diffusion to nearly equal temperature throughout the bar. If the dynamo had been doing no work externally at the time of the accident, the time required to get up a strong enough current out of the dynamo to produce much heating effect would have been very much longer than it was. The result to Lord Armstrong might not have been very noticeably different from what it was, but the attendant's fingers would have been burned also.

where $\int ds$ denotes integration all round the border of the cross-section. Hence the condition for constant total amount of current is simply

$$\int ds \frac{d\zeta}{dr} = 0 .$$

For the case of circular cross-section with uniform electric conductivity in all parts of it; and with the circuit-completing conductor either a coaxial, cylindric sheath, or a conductor of any form whatever, provided only that no part of it is near enough to the considered part of the given conductor to sensibly disturb the distribution, if the current, through its circular cross-section, from being of equal current-density at equal distances from the axis, the condition for constancy of total amount becomes simply

$$\frac{d\zeta}{dr} = 0 ,$$

at the boundary of the conductor, where r denotes distances from the axis. The full numerical solution of this problem, from the instantaneous commencement of a current of given total strength (which must necessarily be in the very outer skin, and must require an infinite current for the first instant) through the whole time until the current becomes as nearly as may be uniform throughout the cross-section, is particularly easy, but must be reserved for a future communication. It is identical with the following particular case of Fourier's thermal problem:—Let a given quantity of heat be initially distributed uniformly through an infinitely thin surface-layer of a solid cylinder coated with an impermeable varnish; it is required to find, for any subsequent time, the temperature at any distance inwards from the surface of the cylinder.

On the Segmentation of the Nucleus of the Third Cranial Nerve. By Dr Alexander Bruce. (With Two Plates.)

(Read July 15, 1889.)

The circumstance that the oculomotor or third cranial nerve supplies no fewer than seven muscles of the eye, two of them intrinsic and five extrinsic, and the fact that these muscles may be paralysed either singly or in groups as the result of central lesions, naturally suggest that each muscle is represented in the nucleus by a separate group of nerve cells. Hitherto, however, the anatomical text-books have been content with figuring the nucleus as a small indefinite group of cells lying in the anterior portion of the grey matter surrounding the aqueduct of Sylvius.

The well-known experiments of Hensen and Völckers (*Graefe's Archiv*, Bd. xxiv., 1878, p. 1) would appear to have been the first attempt to investigate the question. They exposed the nucleus in the dog, and by stimulating its various portions with electricity, they came to the conclusion that the centres of the various muscles were arranged from above downwards in the following order:—

(1) Tensor choroideæ (accommodation).
(2) Sphincter iridis.
(3) Rectus internus.
(4) Rectus superior.
(5) Levator palpebræ superioris.
(6) Rectus inferior.
(7) Obliquus inferior.

In 1881 Kahler and Pick (*Prag. Zeitschrift f. Heilkunde*, Bd. ii. p. 30), from the observations of two clinical cases of paralytic lesions of the muscles, were led to adopt a somewhat different, and what appears *a priori* to be a more natural arrangement. They placed the centres in a median and a lateral group, as follow:—

Median Group.	*Lateral Group.*
Tensor choroideæ.	... ...
Sphincter iridis.	... ...
Rectus internus.	Levator palpebræ superioris.
... ...	Rectus superior.
Rectus inferior.	Obliquus inferior.

This appears a more natural grouping, from the fact that the three elevators of the eye and eyelid, which habitually act together, are placed in juxtaposition, whereas, as has been pointed out by Manthner in his *Nuclearlähmung*, Hensen and Völckers placed a depressor, the rectus inferior, between two elevators, thus separating it also from its associated centre, that for superior oblique. Kahler and Pick found in one case in which the elevators were paralysed that the most external of the hinder (or lowermost) roots of the third nerve had degenerated, and in another case of paralysis affecting the rectus internus that the postero-median rootlets were degenerated. Manthner (*Nuclearlähmung*) is inclined to accept the arrangement of nuclei just described. Von Gudden (*Tgbltt. der Naturf. u. Aerzte*, Salzburg, 1880, p. 100; and "Mittheilungen der Morph. physiolog. Gesellschaft zu München," *Aertzl. Intellig. Blatt.*, 1883) found in the rabbit two nuclei on each side, a ventral and a dorsal. The ventral or anterior was connected with the nerve of the same side, the dorsal or posterior with that of the opposite side. Thus the right third nerve would arise from the right ventral and the left dorsal nucleus. He further divided the ventral into two parts, an anterior (*i.e.*, nearer the cerebrum ?) and a posterior portion.

Merkel (*Graefe Saemisch. Handbuch d. Ges. Angenheilkunde*, i. p. 135) and Vulpian and Philippeaux (*Essai sur l'origine de plusieurs paires de nerfs crâniens*) admit a decussation of some of the fibres of the third nerve, but Duval denies its existence.

Edinger (*Westphal's Archiv*, 1885, p. 858) describes a median nucleus lying between the two principal nuclei, which sends fibres to each of the two nerves. Darkschewitsch (*Neurolog. Centralblatt*, 1886, No. 5) describes a small-celled nucleus at the upper extremity of the posterior longitudinal fasciculus, which he regards as probably forming part of the oculomotor nucleus.

Westphal (*Arch. f. psychiatrie*, xviii. p. 847) found in a case of ophthalmoplegia externa that the main part of the oculomotor nucleus was degenerated, while two other groups of nuclei remained normal. One of these lay a little to the side of the raphé, and is named medial by Westphal. The other, or lateral, lies somewhat external to the posterior extremity of the medial nucleus. He apparently does not recognise a nucleus altogether in the median plane. (Westphal's paper contains a very exhaustive account of the

symptomatology and description of the microscopic examination of the nucleus.)

A considerable body of evidence has thus accumulated in favour of an anatomical segmentation of the nucleus in question, but no two authors seem to be agreed on the position and number of the segmented groups. To investigate the point further, I made a series of transverse vertical sections of the brain of a human fœtus, from the lower border of the fourth to the upper margin of the third nucleus; and a second series, from an adult human brain, of longitudinal transverse sections through the same nuclei. The first series is the more instructive. The divisions of the nuclei are represented in figures 1–8; one of the longitudinal transverse sections is shown in figure 9. From these and the special description of the plates which follows, it will be seen that the nucleus of the nerve to the superior oblique muscle which lies below the most inferior part of the nucleus of the third nerve bears the same relation to the posterior longitudinal fasciculus as does the latter nucleus, but is always to be distinguished from it by the direction taken by its root fibres, which pass outwards and backwards along the border of the central grey matter and the formatio reticularis, while the rootlets of the third nerve always arise from its anterior (ventral) surface, and pass at once directly forwards.

Coming to the nucleus under special consideration, we distinguish the following groups:—

A. An anterior group.
B. A postero-external group.
C. A median nucleus.
D. A postero-internal nucleus.
E. A superior nucleus.

A. The anterior or ventral group, which extends along the greater part of the nucleus (being absent only at its extreme upper termination), lies in relation to the innermost part of the main body of the posterior longitudinal fasciculus, and bears the same relation to it throughout. It is distinctly segmented, and may be divided into an *inferior nucleus* (fig. 2, III. *i.n.*), characterised by the absence of commissural fibres between the two sides, and a *main nucleus*, which is again capable of being subdivided into at least three parts,

in virtue of its relation to the median nucleus. Of these three, the *lowest* (fig. 3) lies below the level of the median nucleus, and has a highly developed system of commissural fibres; the *intermediate* (fig. 4, and fig. 5, III. *a.n.*) lies at the same level as the median nucleus, and has also, at least in its lower portion, a well-developed commissural system; while the *upper* division lies at a higher plane than the median nucleus, and has extremely scanty commissural connections with its fellow (figs. 6 and 7). The anterior nucleus attains its largest dimensions at its lowest extremity (figs. 3 and 4), the point at which the commissural system is most highly developed. It gradually diminishes in size as it ascends. It is seen on longitudinal transverse sections to be imperfectly segmented from what I have termed the inferior nucleus; this is of somewhat smaller size than the adjacent portion of the anterior group. The upper extremity of the nucleus under consideration is seen to fade gradually away, its volume and the number of cells diminishing almost *pari passu.* It terminates altogether at a slightly lower plane than does a group of cells to be afterwards described as the superior or small-celled nucleus.

B. The postero-lateral group is formed of motor cells of the same size and type as those of the anterior group. It lies on the posterior surface of the longitudinal fasciculus, near its outer extremity. Its length is considerably less than that of the anterior group. Below, it begins slightly above the inferior extremity of the median nucleus; while above, it is replaced by the superior or small-celled nucleus. It begins, below, as a small circular group of nerve cells, distinctly separated from the anterior group (fig. 5, *p.l.n.*), and gradually increases in size upwards, attaining its maximum near its upper extremity (fig. 6, *p.l.n.*). I have given the name of *external nucleus* (fig. 5, III. *l.n.*) to a group situated external to the middle and upper part of the postero-lateral group, partly between, and partly outside the longitudinal fasciculus. This is much shorter than the postero-lateral nucleus, lying opposite its middle third.

C. The median nucleus (fig. 4, and fig. 5, *m.n.*) lies in the middle line opposite the intermediate portion of the anterior group. Its upper portion (fig. 5, *m.n.*) has the shape of an elongated spindle tapering anteriorly to a sharp point, while the posterior end is somewhat rounded. A line drawn transversely between the centres

of the two postero-lateral nuclei will pass through the dorsal extremity of the median nucleus. The upper portion of the median nucleus is bounded laterally by fine medullated fibres which arise from its nerve cells and pass along its outer surface forwards to enter, partly, the root of the nerve, and partly the posterior longitudinal fasciculus. It is also connected with the adjacent portions of the anterior group by a fine network of medullated fibres. The lower part of the median nucleus (fig. 4) has an irregularly circular outline. It has no lateral boundary of nerve fibres.

D. The postero-internal or pale nucleus (figs. 5, 6, 7, 9, *p.i.n.*) presents in sections of the adult brain which have been stained by Weigert's hæmatoxylin a remarkably pale appearance. It is wedged in between the median and postero-external nuclei (see fig. 5, *p.i.n.*), but extends to a higher level than the former. It terminates above by a large club-shaped head (fig. 9, fig. 7). (This apparently corresponds to the medial nucleus of Westphal.) Its pallor is due to the comparative absence of medullated fibres between, and the large lymph spaces which surround, the nerve cells contained in it. These cells differ from those of the other group, being more oval or rounded, containing large spherical nuclei having fewer processes, and staining of a much fainter brown by Weigert's method.

E. The superior (or small-celled nucleus of Darkschewitsch) lies at the extreme upper extremity of the oculomotor nucleus, forming a small indefinitely circumscribed group at the outer margin of the posterior longitudinal bundle. It deserves a special name from its distinct segmentation from the postero-lateral group and from the small size of its cells. It is imbedded among the fibres of the outer portion of the posterior longitudinal fasciculus, many of the fibres of which evidently terminate in its cells, and it forms the terminus of many of the fibres of the posterior commissure.

Roots of the Third Nerve.—The sections tend to confirm at least partially the statements of Kahler and Pick as to the origins of the nerve. The roots of the postero-lateral and external group of nuclei form the most external fibres of the nerve as it passes through the tegmentum. They constitute, however, the upper as well as the lower external fibres. The anterior nucleus sends its roots into the innermost part of the nerve, while fibres from the median and the

postero-internal nuclei enter the innermost part of the nerve opposite the upper half of the nucleus. I have searched in vain for any decussation of the roots as described by Von Gudden in the rabbit.

While it is certainly premature to express any positive opinion as to the functions of the various nuclei described, there would appear to be ground (see Westphal's case, *loc. cit.*) for the belief that the inferior, anterior, and postero-lateral (including external) groups of nuclei are connected with the extrinsic muscles of the eyeball and the elevator of the eyelids, and that the median and the postero-internal nuclei are the centres for accommodation and contraction of the pupil. If the view of Kahler and Pick be correct, then the postero-lateral group will form the centres for the elevators, while the anterior nucleus will be connected with the internal rectus, and the inferior nucleus with inferior rectus. With regard to the two central groups, the median and postero-internal, the evidence seems to be insufficient for the view which would place the accommodation centre at a higher level than that for the contraction of the pupil. The circumstance that a nuclear lesion seems invariably to affect the accommodation of both eyes, while a similar lesion may undoubtedly affect the sphincter iridis of one side alone, seems best explained by the view that the accommodative act is controlled from one centre, while there are two centres for the contraction of the pupil. If this view be correct, then the median nucleus may form the centre for accommodation and the postero-internal necleus that for pupil-contraction. In this respect the intimate relation of position of the median nucleus to the main portion of the anterior nucleus (the supposed centres for the associated acts of accommodation and convergence) is of interest.

Description of Plates.

Fig. 1 shows the nuclei (IV. *n.*) of the two fourth nerves lying imbedded amongst the outermost fibres of the posterior longitudinal fasciculus (*p. l.f.*). Its root (IV. *r.*) is distinguished from that of the third nerve by passing from the postero-external aspect outwards and backwards, along the anterior margin of the grey matter surrounding the aqueduct of Sylvius. From the inner aspect of the nuclei, fibres (*t.f.*) pass inwards and forwards to enter the bundles of the posterior longitudinal fasciculus. The anterior parts of the two posterior longitudinal fasciculi meet so as to form a process shaped like the capital letter U. Into the anterior surface of this U-shaped process there enter medullated fibres, which arise apparently in the decussating superior

cerebellar peduncles. (The level of the section is at the decussation of these peduncles.)

Fig. 2. Section made almost immediately above the preceding one, through the lowest portion of the nucleus of the third nerve. The inferior extremity of the nucleus (III. *i.n.*) is seen lying upon the dorsal aspect of the posterior longitudinal fasciculus. At this level none of the root fibres are visible, but on the inner aspect there are seen fibres (*i.o.f.*) passing forwards to join the anterior fibres of the posterior longitudinal fasciculus. The two sets of anterior fibres converge towards each other in the shape of the letter V. They appear to be genetically different from the fibres of the main part of the fasciculus, for fine medullated fibres pass from their anterior surface across the middle line, forwards, to disappear either in the superior cerebellar peduncle, or, what is more probable, to terminate in a nucleus situated on the inner side of each of these tracts, or perhaps in a group of medullated fibres lying still nearer the anterior surface. (These are not represented in the drawing.) At this level there are no commissural fibres between the two nuclei, a fact which seems to warrant our separating this as a distinct portion of the nucleus, the inferior oculomotor nucleus.

Fig. 3. Section is made at a level slightly above that of the preceding through that part of the nucleus at which intercommissural fibres (*c.f.*) begin to appear. An oval-shaped nucleus lies on either side dorsally to the posterior longitudinal fasciculus. Fine fibres form a rich network between the two nuclei. A large group of medullated fibres (*a.o.f.*) originates in the interior of the nucleus and passes inwards and forwards, internally to the V-shaped portion of the posterior longitudinal fasciculus. The fibres which arise from the anterior surface of this V-shaped portion pass forwards to the opposite side in a manner similar to that described in Plate II., but appeared now in the section from which the drawing was made, rather to end in a group of fibres immediately anterior and internal to the superior cerebellar peduncle. (In addition to these, other fibres were seen, after crossing the middle line, to bend backwards and outwards and become continuous with the fibres known as the deep fibres of the corpora quadrigemina.) This part of the nucleus (III. *a.n.*) forms the lower part of the anterior group.

Fig. 4. The section is made through the inferior extremity of the median nucleus (III. *m.n.*). The two anterior nuclei still continue to be united by intercommissural fibres. The anterior nucleus is distinctly increased in size. It forms a compact oval-shaped group slightly imbedded in the posterior longitudinal fasciculus. Fibres passing from its inner aspect forwards into the V-shaped portion of the fasciculus are still well seen. In the median line, dorsal to the intercommissural fibres, is the lower extremity of the median nucleus (III. *m.n.*). At this level it has a very indefinite form and outline, and the cells are interspersed by a fine net-work of medullated fibres. Among the fibres of the outer portion of the posterior longitudinal fasciculus, motor cells are scattered in such numbers as to deserve a special name. I have accordingly termed these the external nucleus (III. *e.n.*). The posterior longitudinal fasciculus, both in its posterior or oblique and in its anterior or V-shaped portion, shows a notable increase in the number of its fibres. Part of this is due to fibres entering its anterior aspect from the various sources already indicated, but more markedly to the increase of connections with the higher levels of nucleus on the same side. The condition of the vascular supply of the nucleus in the section from which the drawing was made is of

interest. One artery ran close to the median plane from before backwards into the median nucleus, while another, placed laterally to the above, ran to each of the anterior nuclei. This appearance is not shown in the drawing, owing to want of space.

Fig. 5. The section is made through the centre of the median nucleus (*m.n.*). This is seen to occupy the median plane, and to have the shape of a long spindle tapering more gradually towards its posterior than towards its anterior extremity. It has on either side a bundle of medullated fibres arising among its cells and running forwards towards the V-shaped portion of the posterior longitudinal fasciculus. Some of these fibres evidently arise from this latter structure, while others as certainly pass through it and enter into the innermost root fibres of the nerve. The anterior nucleus (III. *a.n.*) is seen lying upon the posterior surface of the innermost three-fourths of the posterior longitudinal fasciculus. Behind the anterior group lies a second smaller almost circular group of nerve cells, the postero-external (III. *p.e.n.*), of a similar shape and size. This lies behind the outermost fourth of the posterior longitudinal fasciculus. From its anterior surface a bundle of fibres passes inwards and forwards between the median nucleus and the anterior group, insinuating itself between the lateral fibres of the median nucleus and fibres arising from the internal surface of the anterior group. Most of these fibres from the postero-external nucleus seem to terminate in the V-shaped portion of the posterior longitudinal fasciculus of the same side. It is possible that some of them may join the innermost root fibres of the third nerve, but it is certain that none of them cross the middle line to enter the nerve of the opposite side. The root fibres from this nucleus pass through the underlying portions of the posterior longitudinal fasciculus and enter the outermost part of the nerve. A small group of nerve cells (external nucleus, *e.n.*) lies on the outer side of the posterior part of the posterior longitudinal fasciculus. In the wedge-shaped space, left between the posterior part of the median nucleus on the inner side and the posterior nucleus with its ventral fibres on the outer, lies a large nucleus (*p.i.n.*), to which I have given the name of the postero-internal or pale nucleus. It is characterised by its cells being (1) considerably smaller than those of the lateral groups, (2) staining less intensely by Weigert's method, (3) by the cells being surrounded by large lymph spaces, and (4) by their having a comparatively slight intercellular network of medullated fibres. The external nucleus (III. *e.n.*) is seen as a small nearly circular group of cells partly imbedded and partly external to the outermost portion of posterior longitudinal fasciculus (*p.l.f.*).

Fig. 6. A vertical transverse section made above the level of the median nucleus. The anterior nucleus (III. *a.n.*) is greatly reduced in size, being rather less than half the size of the same nucleus in fig. 5. The number of the cells contained in it is diminished in a much higher proportion. The postero-external nucleus, on the other hand, is slightly increased in size. It maintains the same relationship to the posterior longitudinal fasciculus as in the previous section. The two postero-internal or pale nuclei (*p.n.*) have the form of long spindles or ovoids, and are separated from each other by an interval about a third of their width. The nucleus and its cells present the same distinctive characteristics as have been described under fig. 5. Fine medullated fibres pass from the anterior aspect of the three groups of nuclei towards the innermost portions of the posterior longitudinal fasciculus. Some undoubtedly enter the roots of the nerve, others become connected with the fasciculus.

Fig. 7. Vertical transverse section through the lower extremity of the superior nucleus (III. *s.n.*) or "small-celled nucleus" of Darkschewitsch, the upper part of the pale nucleus (III. *p.i.n.*), and of the anterior nucleus (III. *a.n.*). The anterior nucleus (III. *a.n.*), still futher reduced in size, lies internal to the anterior part of the posterior longitudinal fasciculus. Behind and partly internal to the anterior nucleus is seen the pale nucleus (*p.i.n.*), in a position corresponding to that described in the previous section. It is of somewhat larger size, but its component cells show otherwise similar appearances. The upper nucleus (II. *s.n.*) lies imbedded amongst the most posterior fibres of the posterior longitudinal fasciculus. It is somewhat ovoid in shape, the cells are distinctly smaller than those of the posterior nucleus, and there is a somewhat dense outer cellular network of medullated fibres. The posterior longitudinal fasciculus is considerably reduced in size compared with figs. 5 and 6.

Fig. 8. Transverse section through the superior nucleus (III. *s.n.*) *p.l.f.*, the posterior longitudinal fasciculus greatly reduced in size; *p.c.*, the posterior commissure passing forwards to terminate (partly) in the small-celled nucleus.

Fig. 9. Longitudinal section through the III. nucleus. III. *p.i.n.*, the postero-internal nucleus. Its club shape and its pallor are well represented—the large head above the narrower part below. *p.l.f.* posterior longitudinal fasciculus; *p.e.n.*, the postero-external (and probably partly anterior) nucleus; III. *vent.*, third ventricule.

On the Nature of a Voluntary Muscular Movement. By John Berry Haycraft, M.D., D.Sc.

(From the Physiological Laboratory of the University of Edinburgh.)

(Read January 20, 1890.)

(Abstract.)

Histologists have long ago demonstrated that a muscle is not a simple histological unit, and that it consists of innumerable muscle fibres associated together in fasciculi, each muscle fibre being brought into individual relationship with the central nervous system by its own fibre. Physiologists have been too apt to overlook this fact, and have regarded a muscle as a simple indivisible physiological unit. When a muscle or its nerve are stimulated by an instantaneous electrical shock all the fibres contract together, for they are all stimulated in the same way, and at the same time. This is not the case, according to the investigations of the author, when the muscle contracts reflexly or voluntarily, and especially during any prolonged contraction. The individual fibres are then never completely co-ordinated together, and vary constantly in their "pull." They

A. BRUCE ON NUCLEUS OF OCULO-MOTOR NERVE.—PLATE I

Fig. 1

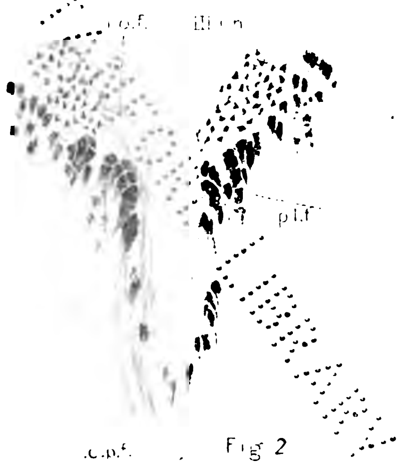


Fig 2

Fig 3

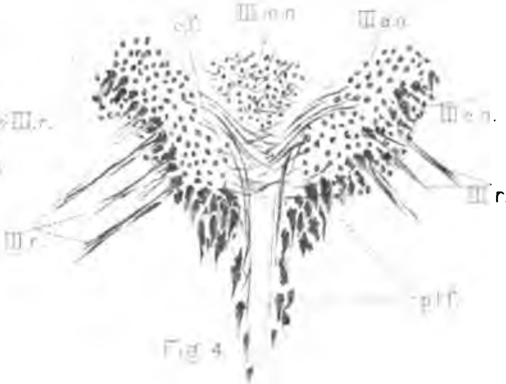


Fig 4

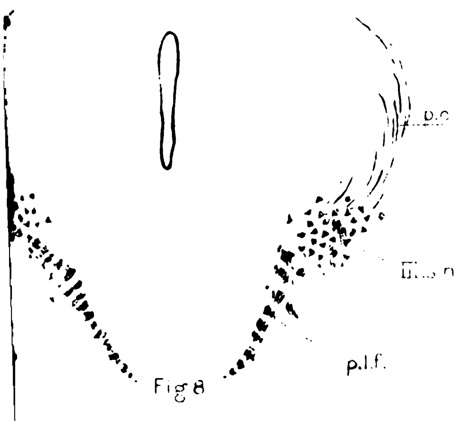


Fig 8

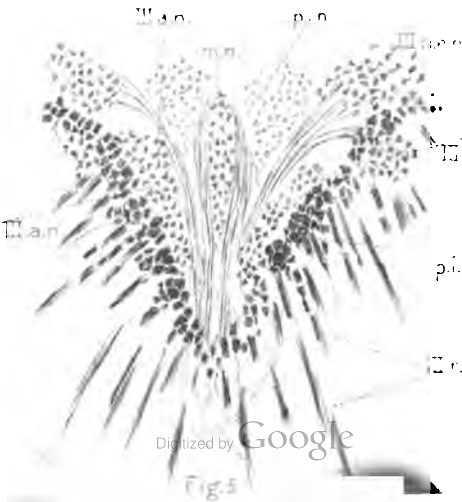


Fig 5

A. BRUCE ON NUCLEUS OF OCULO-MOTOR NERVE.—PLATE II

Fig 9.

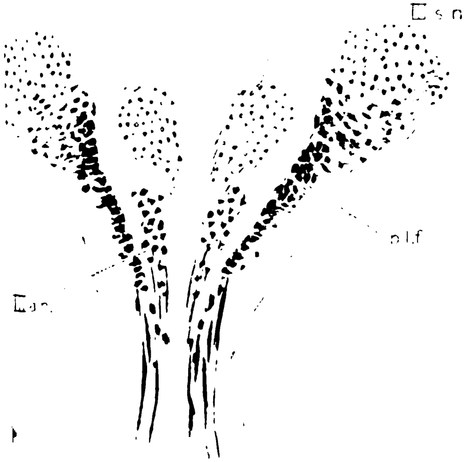


Fig.7.

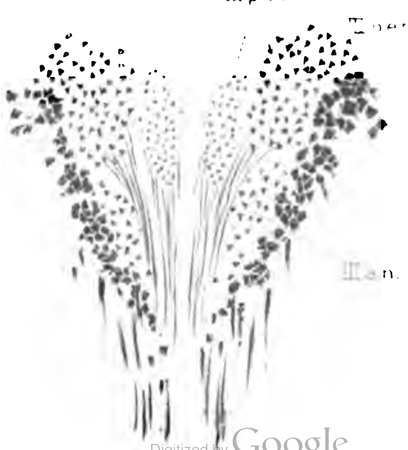


Fig. 8

are like a body of men pulling on a rope where perfect and prolonged co-ordination is impossible. The proof of this is obtained by the use of very delicate levers attached to different parts (fasciculi) of the same muscle. Two levers attached in this way to a muscle always give slightly different curves, evidence of individuality being very apparent. Experiments were conducted both upon the human masseter muscle and upon the gastrocnemius of a frog, and in each case the curves obtained by the two levers corresponding in the main always differed in detail.

It appears that the fibres running in a fasciculus are more co-ordinated together than those running in different fasciculi, and the following experiment shows that the nerve fibres passing from muscle fibres within a fasciculus run together towards the central nervous system in close relationship within the nerve trunk. Make a nerve muscle preparation from a frog, and apply a tiny drop of strong salt solution to one spot on the side of the sciatic nerve. After a minute or so the fibres within one fasciculus will twitch, then those within another, and so on until the whole muscle twitches. This is no doubt due to the gradual invasion of the nerve by the salt solution, the nerve fibres lying together, and therefore stimulated together, all passing to the same fasciculus.

Inasmuch, therefore, as during a voluntary contraction fascicular movements are always going on, it may be that we have not to look any further for the cause of the muscle sound. This, as Helmholtz has shown in an ear resonance sound, would be bound to be set up by such movements, whether occurring aperiodically or at any slow period of their own. Kronecker, Schäfer, Horsley, v. Kries, Landois, and others have figured tracings taken by levers and tambours of muscles contracting under the influence of the will. These tracings always show superimposed upon the main curve tiny, almost periodic, oscillations. These observers have considered the oscillations to indicate that the muscle *as a whole* is in incomplete tetanus during voluntary contraction, due to its receiving a series of nerve impulses breaking one after the other into it. The oscillations are due, however, to an entirely different cause. They are in reality the oscillations proper to the recording instrument which is kept oscillating by the aperiodic fascicular movements due to want of co-ordination. Granted such movements, which the

author has shown to occur, the recording apparatus *must a priori* be kept in oscillation at its own period, just as a swing will be kept swinging when even occasional pushes are given. The complete demonstration of this truth is obtained by changing the recording apparatus, when not only will the oscillations be changed, but they will be found to correspond with the period of the instrument used in every case. This period can be obtained by tapping the instrument, and causing it to record its own oscillations. Loven has obtained variations in the electromotive force of a muscle during voluntary contraction; these variations are described as periodic, and are believed to support the view held as to the interrupted nature of the nerve impulses passing to a muscle. His results differ from those of other observers in respect to the period of his variations, and probably his results are due to the period of the electrometer he used.

If then the muscle sound, the results obtained by Loven, and the tracings obtained by the use of levers, can all be explained by the fact that the fasciculi within a muscle are not completely co-ordinated, there remains no proof that a voluntary muscular contraction is of the nature of a tetanus.

Muscular Contraction following rapid Electrical Stimulation of Central Nervous System. By John Berry Haycraft, M.D., D.Sc.

(*From the Physiological Laboratory of the University of Edinburgh.*)

(Read February 28, 1890.)

(*Abstract.*)

Kronecker, Hall, Schäfer, Horsley, and others have found, when stimulating the spinal cord with rapid induced currents (ten, twenty, thirty, forty times per second), that the muscles always responded by giving a curve showing oscillations of one invariable period, no matter what the period of the stimulation may have been. With rapid stimulation, the muscles in the hands of the first named observers gave oscillations of twenty a second, in the hands of the latter observers of ten a second. These results are to be explained in the following way. When the central nervous system is stimu-

lated, the muscles contract but never smoothly, for local fascicular movement, as the author has elsewhere shown, always occurring. These cause the registering apparatus to oscillate at its own period, just as any swinging body may be kept in motion by occasional disturbances. We can thus explain how each observer obtained the same number of oscillations every time he stimulated the nervous system; it was because he used the same recording apparatus each time. We can also see that the different periods observed by different observers are due to the fact that they each used a different recording apparatus. The author finds that on changing the recording apparatus (lever or tambour) the tracing obtained can be changed at will, and is practically a tracing of the oscillating period of the instrument used.

On one occasion the author was stimulating the upper dorsal region of the cord of a rabbit thirty times a second, and was surprised to find that, in addition to the main oscillations of the tambour just described, there were finer oscillations at the rate of thirty a second. These disappeared on changing the tambour or on slowing the electrical interruptor, or on quickening its speed. They were evidently due to the tambour having some higher overtone of thirty a second at which it could oscillate; why other observers had failed to obtain responses from the muscles synchronous with their stimulation period was because their levers and tambours were unable to respond. While the author was unable to obtain *curves* showing a higher period than thirty a second, yet the *stethoscope* furnished evidence that changes go on within a muscle synchronous with stimuli applied at a much greater rate than thirty a second. When the cord or peduncles are stimulated one or two hundred times a second, the muscles respond by giving a note of exactly the same pitch.

The ear gives better evidence of the nature of the muscular response than do such registering apparatus as tambours and levers. Nevertheless, if one is fortunate enough to get a tambour which has itself some period corresponding to the period of stimulation, it is quite capable of registering the effects which follow stimuli applied twenty or thirty times a second. If the tambours do not respond in the way indicated, they merely oscillate at their own slowest period. The oscillations are kept up in this case not only by such of the motions of the muscle, trembling at a quicker rate, as can

compound themselves with the period of the lever, but also by the fascicular motions which always occur during a muscular contraction, and which have been elsewhere described by the author.

Synthesis of Sebacic Acid. By Prof. Crum Brown and Dr James Walker.

(Read May 19, 1890.)

(*Abstract.*)

Potassium ethyl adipate was prepared by adding the calculated quantity of alcoholic potash to an alcoholic solution of adipic ether, and boiling. After the alcohol had been driven off on the water-bath, the residue, when shaken up with water and ether, yielded an aqueous solution which after concentration was used for electrolysis.

The electrolysis was conducted in precisely the same manner as in our former syntheses.* A colourless oil was observed to collect on the top of the solution. After completion of the electrolysis, the contents of the crucible used as cathode were extracted by ether, which was subsequently distilled off and the residual oil kept for some hours at 120°. When hot this oil smells of melted butter, but on cooling the fatty odour disappears, a very slight smell resembling melons being left. The boiling-point of the oil is 307° (uncorr.). Analysis yielded the following results:—

·1452 gr. substance gave ·3470 gr. CO_2
and ·1329 gr. H_2O

	Found.	Calculated for $C_{14}H_{26}O_4$.
C	65·18 %	65·12 %
H	10·16	10·08

The substance has thus the composition of sebacic ether.

A portion of it was saponified by means of alcoholic potash, the potassium salt separating out on boiling. The acid obtained from this salt by precipitation with hydrochloric acid was found to melt at 128°—the melting point of sebacic acid.

A weighed portion of the potassium salt was ignited, and gave the following numbers:—

·3662 gr. salt gave ·1808 gr. K_2CO_3

	Found.	Calculated for $C_{10}H_{16}O_4K_2$.
K	27·88 %	28·06 %

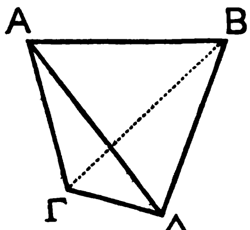
* *Proc. Roy. Soc. Edin.*, 1889–90, p. 54.

On the Relation of Optical Activity to the Character of the Radicals united to the Asymmetric Carbon Atom. By Professor Crum Brown.

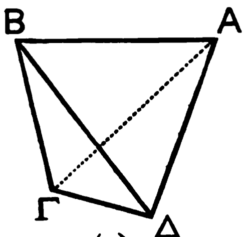
(Read June 2, 1890.)

It is obvious that the amount of the optical activity of a given compound containing an asymmetric atom of carbon depends upon the amount of difference in character among the four radicals united to the asymmetric carbon atom, so that if two of them are very nearly equal we come very near to a compound of a symmetric carbon atom, in which the optical activity is zero. The question suggests itself, How are we to measure this difference of character? We shall assume that there is a function, capable of numerical representation, derivable from the composition and constitution of the radical and the temperature of the substance, and that it is the difference between the values of this function in the case of two radicals which gives us the difference of character referred to. For the sake of brevity, we may call this function the κ of the radical. The object of this paper is to show that, if there be such a function, there are methods by means of which we may hope to ascertain its value in each case.

Let us represent the compound by the formula (C)ABΓΔ, in which (C) represents the asymmetric carbon atom, and A, B, Γ, Δ the radicals united to it *arranged in order of the values of* κ. Now these radicals may be united to the asymmetric carbon atom in either



(1)



(2)

of two ways. One of these ways—we have as yet no means of guessing which—corresponds to right-handed rotation, the other to left-handed rotation. The one form can be changed into the other

by interchanging the positions of any two of the radicals. Now we cannot directly interchange the positions of two radicals by any method such that we can be sure which radicals have been interchanged, but we can act upon one radical so as to increase or diminish its κ, and if, for example, the κ of B is increased so as to be greater than that of A, we have now a new compound, $(C)A_1B_1\Gamma_1\Delta_1$, in which A_1 is the modified B, and B_1, Γ_1, Δ_1 are the original A, Γ, Δ respectively. But we see that we have changed the order in which the radicals (arranged in order of values of κ) are united to the asymmetric carbon atom. If the original compound is represented by (1) in the figure above, the new compound corresponds to (2). If therefore the original compound were right-handed, the new one will be left-handed, and *vice versâ*. The change of sense of rotation is thus an indication that a change has been effected in the value of the κ of a radical such as to change the order of the radicals.

We do not know very many cases of such change, but we know enough to illustrate the foregoing. In right-handed tartaric acid there are two asymmetric carbon atoms, but as these are perfectly similar in all respects we need only look at one. This is then our (C), and our A, B, Γ, Δ, are (arranging them provisionally in order of mass) $-CH(OH)—CO—OH$, $-CO—OH$, $-OH$, and $-H$. By treatment with acetyl chloride, tartaric acid is converted into diacetyl-tartaric acid. Here the radicals are (preserving the order of union to the asymmetric carbon atom) $-CH(O—CO—CH_3)—CO—OH$, $-CO—OH$, $-O—CO—CH_3$, and $-H$. Now the diacetyl-tartaric acid from right-handed tartaric acid is left-handed. We shall assume that addition to a radical increases the value of κ, and that therefore the κ of $-CO—OH$ is greater than that of $-OH$, and that κ is increased when $-OH$ is converted into $-O—CO—CH_3$; making these assumptions, we find then that the κ of $-O—CO—CH_3$ is greater than that of $-CO—OH$. Again, we can convert right-handed tartaric acid into its ethyl ether. Here, preserving the old order, we have the radicals $-CH(OH)—CO—O—C_2H_5$, $-CO—O—C_2H_5$, $-OH$, and $-H$. This ether is right-handed. From it, by the action of acetyl chloride, we can obtain diethyl diacetyl-tartrate. Here we have the radicals $-CH(O—CO—CH_3)—CO—O—C_2H_5$, $-CO—O—C_2H_5$, $-O—CO—CH_3$, and $-H$. This ether is also right-handed, but only to a small extent. We thus see that the κ of $-O—CO—CH_3$ is less

than that of $-CO—O—C_2H_5$, but not much less. Take now the case of dimethyl tartrate. This, like the acid, is right-handed, but yields with acetyl chloride a strongly left-handed dimethyl diacetyl-tartrate. From this we gather that the κ of $-O—CO—CH_3$ is greater than that of $-CO—O—CH_3$, although the two radicals are isomeric.

Let us now consider the case of asparagine. We do not know which of the two formulæ $HO—CO—CH_2—CH(NH_2)—CO—NH_2$, and $HO—CO—CH(NH_2)—CH_2—CO—NH_2$ is that of asparagine; we shall here use the latter.* In potash solution this becomes $K—O—CO—CH(NH_2)—CH_2—CO—NH_2$, if there were no dissociation of the ions. Similarly in hydrochloric acid solution it becomes $H—O—CO—CH(NH_3Cl)—CH_2—CO—NH_2$, also if there were no dissociation of the ions. Here of the two radicals $-CO—OH$ and $-NH_2$ the one is increased by dissolving the substance in potash, the other by dissolving it in hydrochloric acid; and (as is well known) asparagine rotates to the left in alkaline, and to the right in acid solution. The case of aspartic acid is quite similar.

Another case which has been pretty fully investigated is that of active amyl alcohol and its derivatives.† Active amyl alcohol has the constitution $CH(C_2H_5)(CH_3)(CH_2OH)$; it is left-handed, as is also, to nearly the same extent, the corresponding amylamine. But the other derivatives, in which the fourth radical in our formula is increased (as the bromide, $CH(C_2H_5)(CH_3)(CH_2Br)$, the hydrochloride of the amylamine, and still more the di- and tri-amylamines), are right-handed. From all of which we may conclude that the κ of $-CH_2OH$ is less than that of $-C_2H_5$ and greater than that of $-CH_3$; that the κ of $-CH_2OH$ is very nearly equal to that of $-CH_2NH_2$, that the κ of $-CH_2Br$ is greater than that of $-C_2H_5$.

Active lactic acid and its salts have been examined in reference to their optical activity by Wislicenus.‡ He finds that active lactic acid rotates in the opposite sense from its salts—this would mean that the κ of $-COOH$ is less than that of $-CH_3$, so that the in-

* I use these formulæ, and not those in which asparagine is represented as a salt, because I am here dealing only with asparagine in alkaline or acid solution.

† See Plimpton, *Chem. Soc. Jour.*, xxxix. 332; and Just, *Liebig's Ann.*, ccxx. 146.

‡ *Liebig's Ann.*, clxvii. 302.

crease of the κ of $-COOH$ by its change into $-COOM$ brings it above the κ of $-CH_3$, and changes the order of the radicals. If this be so, then of the three radicals

$$\begin{array}{c}H\\|\\-C-H,\\|\\H\end{array}\quad \begin{array}{c}H\\|\\-C-O-H,\\|\\H\end{array}\quad \begin{array}{c}O\\\|\\-C-OH,\\ \\ \end{array}$$

the κ is greatest in the second and least in the third, so that the replacement of H by OH raises the value of κ, the replacement of H_2 by O lowers it, and lowers it more than the replacement of H by OH raises it.

There is one set of cases to which we cannot as yet apply the method; viz., where the asymmetric carbon atom forms part of a ring, as in diacetyl-tartaric anhydride, asparagine (if we regard it as a salt with twice the formula given above), camphor, &c.

Of course we cannot as yet even approximate to a formula for the amount of rotation in terms of the four κ's and temperature, but as the rotation becomes zero when any two κ's become equal we may presume that it contains the product of the differences of the κ's.

The first thing to be done with this speculation is to find whether κ is really a function of the composition and constitution of the radical and of the temperature of the substance, or varies with the character of the other three radicals. This will require the preparation and careful examination of many active substances and their derivatives.

June 21, 1890.

Since the paper of which the above is an abstract was read to the Society I have seen a note on the same subject by Mr Philippe A. Guye, published in the *Comptes Rendus* for March 31st of this year.

There is a good deal common to Mr Guye's view and mine, and I take this opportunity of acknowledging his priority in the points which are common.

The chief difference is that Mr Guye regards the *mass* of each radical and the distance of its centre of gravity from the centre of figure of the tetrahedron as all that has to be considered as determining the amount of asymmetry, whereas I suppose κ to be a function of the composition and constitution of the radical, not necessarily proportional to the mass and the distance of the centre of gravity from the centre of the tetrahedron, so that, for example,

the replacement of H_2 by O diminishes κ although it increases the mass of the radical.

On the Mean Level of the Surface of the Solid Earth. By Hugh Robert Mill, D.Sc.

(Read June 2, 1890.)

In a paper "On the Height of the Land and the Depth of the Ocean," read to the Society on 19th December 1887, and published in the *Scottish Geographical Magazine*, vol. iv. pp. 1–41, Dr John Murray gives a very detailed estimate of the volumes of oceanic hollows and continental protuberances. I have recently had recourse to Dr Murray's calculations in order to determine the position of the contour-line which should separate the portions of the solid earth above the general level of the surface from those below that level. The results of this determination were published in the *Scottish Geographical Magazine*, vol. vi. pp. 182–187; but since then the theoretical importance of the particular contour-line in question has induced me to give further consideration to the subject.

The term "lithosphere" has been applied to the solid part of the earth, and "hydrosphere" to the oceans collectively, corresponding etymologically with "atmosphere" by which the gaseous envelope is familiarly known. If the lithosphere were smooth and the earth at rest, it would be entirely and uniformly covered by the hydrosphere. The amount of dry land emergent above the surface of the hydrosphere depends in the first instance on the configuration of the lithosphere, in the second on the volume of the hydrosphere.

The surface of the hydrosphere is usually adopted as the standard level from which heights and depths are estimated; theoretically this is unsatisfactory, on account of the distortion of surface caused by the gravitational attraction of the ridges on the lithosphere. The only perfect reference surface is that of the geoid, or smooth figure which would result if the ridges of the lithosphere were laid to rest in the hollows. In order to arrive at this mean level of the lithosphere (shortly mean-sphere level), the requisite data are the volumes of the hydrosphere and of the emergent land, together with the areas occupied by each. From calculations given at length in

the paper already cited, I was led to fix the position of mean-sphere level at about 1400 fathoms below sea-level; that is to say, nearly midway on the very steep descent which everywhere separates the comparatively flat-topped world-ridges from the still flatter-bottomed world-hollows. My friend Mr J. G. Bartholomew has drawn my attention to the fact that recent hydrographic exploration in the Pacific and Indian Oceans has revealed the existence of a far greater area more than 3000 fathoms deep than was supposed to exist when the maps for Dr Murray's paper were constructed. Hence in that paper the area with depths between 2000 and 3000 fathoms must be restricted, and the area with depths greater than 3000 fathoms enlarged to the same extent. The other contour-lines are practically unaltered, but the volume of the hydrosphere is greater than was assumed, and the contour-line of mean-sphere level must lie at a somewhat greater depth than 1400 fathoms.

It is obvious that the two areas into which the mean-sphere level line divides the surface of the globe bear no necessary relation of size to one another. If, for example, the elevated area were a vast pillar 1 square mile in section, the mean-sphere level line would be traced round it very near the base; if, on the other hand, the elevated area were a mass 166,000,000 square miles in section, mean-sphere level would be close to the mouth of the narrow pit which formed the depressed area.

When a map of the sphere on an equal surface projection is cut out along the contour-line of 1400 fathoms of ocean depth (assumed mean-sphere level), the area above that level is, as estimated by weighing the two portions, 20 per cent. smaller than the area beneath that level. In this experiment, the red line marking mean-sphere level on the map was visible on the portion representing the depressed area. This line was 80 inches long, and assuming that $\frac{1}{80}$ of an inch were left on the one side, this would account for one quarter of the difference, leaving the depressed area 15 per cent. larger than the elevated area; *i.e*, the two areas would be equalised by transferring $7\frac{1}{2}$ per cent. of the larger to the smaller. In so rough an experiment it is not a great assumption to take the two areas as equal, but if instead of doing so we inquire into one or two other ratios, the equality in area of the elevated and depressed region is practically proved.

Using Dr Murray's corrected figures,* we get—

Area of lithosphere above contour-line of 1000 fathoms,			78,700,000 sq. miles	
,, below	,,	,,	118,200,000	,,

Or the ratio is 1 : 1·5.

Area of lithosphere above contour-line of 2000 fathoms			= 109,200,000 sq. miles	
,, below	,,	,,	= 87,700,000	,,

Or a ratio of 1 : 0·8.

And by my experiment just cited—

Area of lithosphere above contour-line of 1400 fathoms,			89,000,000 sq. miles	
,, below	,,	,,	107,600,000	,,

Or a ratio of 1 : 1·2.

As previously pointed out, recent researches show that mean-sphere level should be placed somewhat deeper than 1400 fathoms, and if this depth were 1700 fathoms the result would almost exactly correspond to an equalisation of areas.

Pending the remeasurement of large-scale maps on which the results of the exploration of the Pacific at present in progress by British surveying ships can be represented, and pending also fuller knowledge of the relief of polar regions and of the gravitational distortion of the hydrosphere, it is imprudent to do more than point out the position of mean-sphere level as somewhere between 1400 and 2000 fathoms beneath mean sea-level. At the same time it is to be noted that some contour-line between 1400 fathoms and 2000 fathoms (probably that of 1700 fathoms) divides the surface of the lithosphere into two equal areas—one of elevation, one of depression.

This coincidence can hardly be accidental, and although at present I do not see how the fact of the volume of the projecting portion of one half the lithosphere surface being equal to the volume of the depression occupying the other half bears upon physical geography and geology, the question demands, and will probably repay, investigation. If the substance of the globe were incompressible and elastic, it would follow that the depression of one half the surface would elevate over the other half a protuberance equal in volume to the depression. The depressed area is the region termed by Dr Murray the "abysmal area," and has distinct physical

* *Scot. Geog. Mag.*, 1890, vol. v. p. 265.

properties unlike those of the other half of the world. If it were possible to show that one half of the earth's surface was subjected to a compressive stress from which the other half was free during the period of plasticity, the magnitude of the pressure would be measured hydrostatically by the mass elevated over the free half. Except the pressure of the hydrosphere on the abysmal area, no such force is at present at work, and that pressure is certainly quite inadequate to the result.

A Geometrical Method, dependent on the Principle of Translation. By David Maver. (With Plate.)

(Read March 3, 1890.)

The properties of generating lines and surfaces often afford an easy and beautiful mode of demonstrating geometrical theorems which, if done by the ordinary method, would be long and tedious. As the subject, so far as the writer is aware, is new, a short paper upon it may not be unacceptable.

The following principles will readily be admitted :—

1. Let AB and CD be two parallel straight lines, and EF, GK any lines whatever drawn from the line AB to the line CD. If these lines EF, GK advance equal distances along AB and CD, each keeping parallel to itself, the spaces passed over by these lines will evidently be equal. Let these spaces be represented by *s*EF and *s*GK, which may be read space generated by EF and space generated by GK, then we have *s*EF = *s*GK. From this it follows that if one side of a triangle be the direction of motion the spaces generated by the other two sides are equal, that is if ABC be a triangle and BC the direction of motion *s*AB = *s*AC.

2. If there be two lines EF, GK standing upon the straight line FK, and if FK be the direction of motion, and *s*EF = *s*GK, then the lines EF, GK are between the same parallels, that is if EG be joined, EG is parallel to FK.

3. If AB and CD be two equal straight lines, and equally inclined to their respective directions of motion, we plainly have *s*AB = *s*CD.

4. If $AB = CD$ and $sAB = sCD$, or if $pAB = qCD$ and $psAB = qsCD$ then AB and CD are equally inclined to their respective directions of motion.

5. If $sAB = sCD$ and AB and CD are equally inclined to their respective directions of motion then $AB = CD$, or generally if $psAB = qsCD$ then $pAB = qCD$.

It would appear, therefore, that of the three conditions, equality of straight lines, equality of the spaces generated by these lines, and equality of their inclinations to their respective directions of motion, if any two are given the third is also given.

There are other properties of generating lines which might be noticed, but to do so, and show their application to the solution of geometrical theorems, would extend this paper to too great a length. We shall therefore proceed to apply what has already been gone over.

BA and BC (fig. 1) are two tangents to the circle EAC whose centre is D. If the diameter CDE be drawn and EA and DB joined, EA shall be parallel to DB. It is easily seen that the angle ABD $=$ CBD. Let DB be the direction of motion then, $sED = sCD = sCB = sAB$, $\therefore$ since $sED = sAB$, EA is parallel to DB (2).

Let the sides AB, BC (fig. 2) of the triangle ABC be bisected by the straight lines CE, AF intersecting in G, then shall $CG = 2EG$ $AG = 2FG$; and if BG be drawn and produced to D, $AD = CD$. Let AF be the direction of motion, then $sCG = sCF(1) = sBF = sBA = 2sEA = 2sEG$, $\therefore$ since $sCG = 2sEG$, $CG = 2EG$ (5). In the same way $AG = 2FG$. Again, if DB be the direction of motion, we have $sAD = sAG = 2sFG = 2sFB = sCB = sCD$, $\therefore$ since $sAD = sCD$, $AD = CD$.

ABCD (fig. 3) is a quadrilateral, and the diagonal BD bisects the diagonal AC in E. If the opposite sides be produced to meet at F and G we have the following results:—(1) If $GD = mAD$ then shall $GB = mCB$, $FD = mCD$, and $FB = mAB$. (2) If BD be produced it will bisect FG in K. (3) AC is parallel to FG.

(1) Take DB as the direction of motion, then because $GD = mAD$ $\therefore sGD = msAD = msAE = msCE = msCB$; but $sGD = sGB$, $\therefore sGB = msCB$, and $GB = mCB$ (5).

To show that $DF = mCD$. Because $GD = mDA$ $\therefore GA = mDA + DA = (m+1)DA$. Taking FB as the direction of motion, we have

$sGA = (m+1)sDA = (m+1)sDF$; but sGA is also equal to $sGB = msCB = msCF$, $\therefore$ $(m+1)sDF = msCF$, and $(m+1)DF = mCF$ (5). Hence $(m+1)DF - mDF = mCF - mDF = m(CF - DF) = mCD$; and $(m+1)DF - mDF = DF$ $\therefore$ $DF = mCD$. That $FB = mAB$ may be shown in the same way as that $GB = mCB$.

(2) FG is bisected in K. Take KB as the direction of motion, then $sGK = sGD = msAD = msAE = msCE = msCD = sFD = sFK$, $\therefore$ since $sGK = sFK$ we have $GK = FK$.

(3) AC is parallel to FG. Because $GD = mDA$ $\therefore$ $GA = mDA + DA = (m+1)DA$. In the same way $FC = (m+1)DC$, and taking AC as the direction of motion, $sGA = (m+1)sDA = (m+1)sDC = sFC$, $\therefore$ since $sGA = sFC$, AC is parallel to FG.

Let AB, CD, EF, GK be four straight lines, and $AB \cdot CD = EF \cdot GK$. If CD and GK are generating lines, and equally inclined to their respective directions of motion, we plainly have $ABsCD = EFsGK$; and conversely, if $ABsCD = EFsGK$ then $AB \cdot CD = EF \cdot GK$. It is easy to see that this truth is general whatever number of factors we may have, and that if $A \cdot B \cdot C = D \cdot E \cdot F$ then $A \cdot BsC = D \cdot EsF = A \cdot CsB = D \cdot FsE = C \cdot BsA = F \cdot EsD$.

The converse is also evidently true, that if $A \cdot BsC = D \cdot EsF$, and the generating lines C, F be equally inclined to their respective directions of motion, then $A \cdot B \cdot C = D \cdot E \cdot F$.

For the sake of brevity it will be convenient to use some symbol to indicate the direction of motion. In the expression $sAB(\overline{CD})$ the symbol is $(\overline{CD})$, which indicates that the direction of motion of the generating line AB is CD.

To employ the properties of generating lines in geometrical demonstration, it is necessary to observe those angles which are equal, and to remember that if the angle made by the generating line with the direction of motion be changed into its supplement the space generated is unaltered.

CA and CB (fig. 4) are two tangents at the extremities of the chord AB of the circle ABF, and CF a secant cutting the convex circumference in D, the chord AB in E, and meeting the concave circumference in F; to show that CF is divided harmonically in D and E. Join FA, FB, BD. It is easy to see that the angle $DBE = DFA$ in the same segment, $CBD = BFE$ in the alternate segment, and FAC the supplement of EBF.

$$CF_s ED(\overline{DB}) = CF_s EB = EB_s CF(\overline{FA}) = EB_s CA = CA_s EB(\overline{BF}) =$$
$$CA_s EF = CB_s EF = EF_s CB(\overline{BD}) = EF_s CD\,,$$

$\therefore$ since $CF_s ED = EF_s CD$, and that the generating lines ED and CD are equally inclined to DB the direction of motion, the angles EDB, CDB being supplementary, we have $CF \cdot ED = EF \cdot CD$.

From the extremities of the base BC (fig. 5) of the triangle ABC are drawn any two lines BD, CE, meeting the opposite sides AC, AB, or those sides produced in D, E, and intersecting in F: to show that $AB \cdot AC \cdot FE \cdot FD = FC \cdot FB \cdot AE \cdot AD$.

$$AB \cdot AC \cdot FE_s FD(\overline{DC}) = AB \cdot AC \cdot FE_s FC = AB \cdot FE \cdot FC_s AC(\overline{CE}) =$$
$$AB \cdot FE \cdot FC_s AE = AB \cdot FC \cdot AE_s FE(\overline{EB}) = AB \cdot FC \cdot AE_s FB =$$
$$FC \cdot AE \cdot FB_s AB(\overline{BD}) = FC \cdot AE \cdot FB_s AD\,.$$

It will be seen that the first generating line FD in the foregoing series of equal products is inclined to its direction of motion at the same angle as the last generating line AD. Hence

$$AB \cdot AC \cdot FE \cdot FD = FC \cdot FB \cdot AE \cdot AD.$$

ABCD (fig. 6) is a quadrilateral inscribed in a circle, the diagonals AC, BD intersecting at E, to show that $AB^2 \cdot CE \cdot DE = CD^2 \cdot AE \cdot BE$. The angle $DCE = ABE$, being in the same segment, also $CDE = BAE$.

$$AB^2 \cdot CE_s DE(\overline{EC}) = AB^2 \cdot CE_s DC = AB \cdot CE \cdot DC_s AB(\overline{BE}) =$$
$$AB \cdot CE \cdot DC_s AE = AB \cdot DC \cdot AE_s CE(\overline{ED}) = AB \cdot DC \cdot AE_s CD =$$
$$DC \cdot AE \cdot CD_s BA(\overline{AE}) = DC \cdot AE \cdot CD_s BE = CD^2 \cdot AE_s BE\,,$$

$\therefore$ since the first and last generating lines DE, BE are equally inclined to their respective directions of motion, we have

$$AB^2 \cdot CE \cdot DE = CD^2 \cdot AE \cdot BE\,.$$

Let the opposite sides BC, AD (fig. 7) of the circumscribing quadrilateral ABCD touch the circle at the points G, E. It is known that if GE be drawn it will pass through P, the intersection of the diagonals AC, BD. Show that $CB \cdot PG \cdot AP \cdot DP = DA \cdot PE \cdot PB \cdot CP$.

$$CB \cdot PG \cdot AP_s DP(\overline{PA}) = CB \cdot PG \cdot AP_s DA = CB \cdot PG \cdot DA_s PA(\overline{AE}) =$$
$$CB \cdot PG \cdot DA_s PE = CB \cdot DA \cdot PE_s PG(\overline{GB}) = CB \cdot DA \cdot PE_s PB =$$
$$DA \cdot PE \cdot PB_s CB(\overline{BP}) = DA \cdot PE \cdot PB_s CP\,,$$

∴ since the inclination of the generating line DP to its direction of motion PA is equal to that of CP to its direction of motion BP, we have CB·PG·AP·DP = DA·PE·PB·CP.

By applying the properties of generating lines to the products of three or more straight lines, as has been done in the last three examples, any number of geometrical results may be obtained. But for a fuller development of the subject, the writer would refer to his work, *Parallel Translations of Lines and Surfaces*, published by A. Brown & Co., Aberdeen, in which the principles are extended and applied to other parts of geometry.

Graphic Records of Impact. (*Abstract.*) By Professor Tait.

(Read July 7, 1890.)

The apparatus for Impact experiments, which was exhibited to the Society on 20th February 1888, has been greatly improved by the substitution of a very true slab of plate-glass, thinly covered with printing-ink, for the sheet of cartridge paper. The record is made by a needle-point which projects from the falling body, and which is kept in constant contact with the plate by means of a light spring. The time of rotation of the plate is given by a tuning-fork, with a small bristle attached, which is kept in vibration by a periodic current, and records alongside of the other tracings.

When the paint is dry, the plate is used as a photographic negative. To correct for possible shrinkage of the photographic paper, circles of known diameter are described on the inked plate at various places, and tested when the fixing process is complete.

The following data give a general idea of the results of a 4 foot fall:—the falling body being a block of hard wood, of 5½ lbs. mass, which impinges on a cylinder of 32 mm. diameter and 32 mm. in length, half of which is imbedded in a large mass of lead cemented to the asphalt floor. The time is less for more violent impacts.

Material of Cylinder.	Compression in mm.	Time of Impact.
Vulcanized India-Rubber, . .	11·5	0s·0077
Vulcanite, ...	2·3	0·0014
Cork, ...	19·0	0·0166
Plane-tree, ...	1·9	0·0018

D. MAVER ON PRINCIPLE OF TRANSLATION.

Fig. 1.

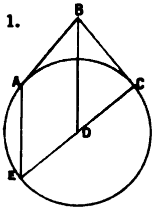


Fig. 2.

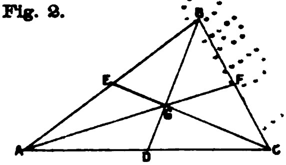


Fig. 3.

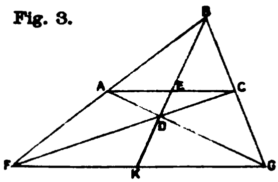


Fig. 4.

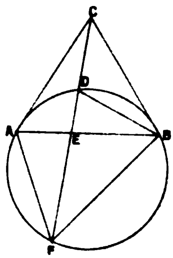


Fig. 5.

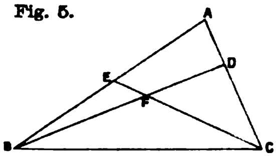


Fig. 6.

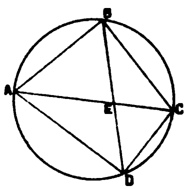
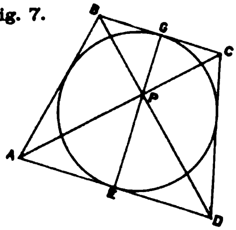


Fig. 7.



On the Number of Dust Particles in the Atmosphere of certain Places in Great Britain and on the Continent, with Remarks on the Relation between the Amount of Dust and Meteorological Phenomena. By John Aitken, F.R.S. (With 3 Plates).

(Read February 3, 1890.)

The portable dust-counting apparatus described in a previous communication to this Society* was designed with a view of making observations on the air at situations where it would be inconvenient to work with the larger laboratory apparatus; and also to enable these observations to be made under conditions more favourable for avoiding local impurities than is possible when working in a house. As the construction of this portable apparatus was completed just as I was about to start for the Continent, the opportunity seemed a favourable one for continuing the investigation into the number of dust particles in the atmosphere, and extending our knowledge on the subject in other countries. The portable apparatus is reduced to such dimensions that it adds but little to one's personal luggage, and can be easily carried to the place where the observations are to be made.

This new form of apparatus has been found to do its work satisfactorily. If a new set of apparatus were made, the only alterations the past experience would suggest is to omit the smallest of the stopcock measures, because I have never as yet had occasion to measure outside air so full of particles that the testing could not have been done with the second smallest measure. The smallest measure should therefore be only retained if the apparatus is required to test air extremely full of particles. For air of the country comparatively free from pollution, all the stopcock measures may be omitted, as they are never required. When the air is pure large quantities of it require to be sent into the receiver, and the air-pump alone is used. When the air is fairly pure, such as country air, 5 or 10 c.c. are generally required for each test.

The silver counting stage of the apparatus was at one time found

* *Proc. Roy. Soc. Edin.*, vol. xvi.

to be troublesome, frequent polishings being required to keep it in good working order. Other metals less easily tarnished than silver have been tried, but with no very satisfactory result as yet. Platinum does very well, and keeps free from tarnish, but it is difficult to give it a very perfect polish. Platinoid, which owing to its property of keeping bright in impure air, promised to bridge this difficulty, but on trial it was found to be most unsuitable, as it soon got spotted all over. The little drops of rain seemed to eat into it, and not more than one test could be made without repolishing. Blackened glass had also been tried again, in the hope it might be got to work well, as it could be much more easily kept in order than silver. Unfortunately it does not act very satisfactorily, owing to the tendency of glass to get dewed. As this dewing is not easily observed in the instrument, it gives rise to difficulties, as the drops are not visible on the dewed surface. And when the surface is kept undewed the drops tend to evaporate, and the number counted is rather low. Further, owing to less light being reflected by the glass surface than by the silver, the drops are not so brilliantly illuminated and therefore not so easily counted. On the other hand, silver has been found to require very little attention in pure air. The stage has frequently been used for weeks without once being taken out of the receiver for polishing, and it worked quite satisfactorily at the end of the time.

The first place on the Continent at which regular observations were made with the dust-counter was Hyères in the south of France. Hyères is a small town on the shores of the Mediterranean, and is the westmost of the health resorts on the French Riviera. It is situated on the southern slopes of a hill, at rather more than two miles from the sea. A flat fertile plain extends for some distance from the town seawards, and at some places the shore is fringed by marshy lands. Observations were occasionally made at an open window of the hotel, which is situated just on the outskirts of the town. The observations of most value, however, were made on the top of Finouillet, a hill situated at about two miles to the north of west of Hyères. Finouillet has an elevation of slightly less than 1000 feet.

Along with this paper is given a table showing the results obtained at this situation, and also at a number of other places on the Conti-

nent and in this country. In the table is entered the number of dust particles, per cubic centimètre, observed in the air of the place, at the hour and date given, each of the numbers given is an average of ten tests made in rapid succession. The direction and force of the wind is also entered, as well as the temperature and the humidity. The humidity is indicated by the amount to which the wet bulb was depressed below the dry one at the time the tests were made. These records of humidity are not very satisfactory, owing to the difficulty of getting suitable positions for making the observations at the different places. In a few cases the observations were made with wet and dry bulb thermometers placed at a window, and in shade and protected as much as possible from radiation. In some cases the humidity was taken with a hygroscope, and its readings converted into wet bulb depressions by comparative readings of the two instruments. In the second last column of the table is entered the state of the air with regard to transparency. These transparency observations are far from satisfactory, as no special means have been adopted of measuring it. It has simply been judged by the unaided eye, which is a very rough method of estimating it. There are many things which alter the apparent transparency of the air, and make the estimate taken in this way very unsatisfactory. For instance, when the sky is clouded all over the air looks more transparent than under a cloudless sky, because the haze in the air between us and the distant scene is not illuminated by direct sunlight, and therefore does not show so much. Again, when looking in the direction away from the sun, the air appears clearer than when looking in the opposite direction. Only a very rough scale of clearness has therefore been attempted. Some special instrument would require to be devised before more definite records can be obtained.

It will be seen from the table that a great number of particles was observed in the air on the 19th March, at the open window of the hotel at Hyères. This high number was evidently due to local contamination, the wind at the time blowing from the town. On the 21st the wind was towards the town, and, as will be seen, the air was much freer from dust, the number per c.c. falling from 46,000 to 5000.

The top of Finouillet was selected for testing the air of

the district surrounding Hyères on account of its position and shape. The hill is situated near the centre of a plain; on its south, west, and north sides it rises with steep slopes to a rather fine point. Its top rises therefore into what we might expect to be the pure air of the district. An examination, however, of the table will show that no very low number was obtained at this situation, never less than 3500 per c.c. The reason for this would appear to be that the hill is surrounded on three sides by a highly cultivated plain, which is dotted all over with the houses of the peasantry, and at many places there are villages. The air of this district is therefore very much polluted with the products of combustion, and though the situation where the observations were made is nearly 1000 feet high, and the hill rises abruptly from the plain, yet it is evident that the polluted lower air came up to the top of the hill, being driven up the slopes by the wind. This ascent of the lower air to the tops of hills has been frequently observed, and will be referred to later. The rapid change in the number of particles observed at short intervals, showed that the lower air was rising to the hill top. On the 23rd and 25th the numbers varied greatly owing to the impure lower air being imperfectly mixed with the purer air above. It is no doubt possible that some of this variation may have been due to change of direction of wind bringing air from more or less polluted parts.

It will be observed that on the 4th of April the number of particles at this situation was remarkably great, the number being as high as 25,000 per c.c. This high number was fairly steady during all the tests made on that day. When I had tested the different parts of the dust counter and found no reasonable cause for doubting its indications, it became necessary to look for some outside cause for this very high number. On looking in the direction of Toulon, distant about nine miles, it was seen that the wind was blowing direct from that town, and bringing the products of combustion to the place of observation; the smoke being traced for some distance from the town coming in a straight line towards Finouillet.

On the 29th March observations were made at La Plage with the view of examining the air resting on the sea. These observations were made at three different places near the shore. The first

on the beach, the second at 100 yards inland, and the third at 250 yards from the beach. The wind at the time was directly inshore, but it is possible it may have been polluted, as there are a few dwellings on the islands outside. Two observations were made on the beach at an interval of an hour and a half. The second observation gave twice the number observed in the first. This may have been due to many of the particles having been produced by the waves, and as the waves continued nearly the same, while the wind fell to about one-half, the number of particles of spray in the air would be increased at the time of the second observation. The increase, however, may have been due to pollution from the dwellings on the islands. At both the situations at a distance from the shore the number was less than on the beach. This may have been due to the air with the spray particles being diluted on its passage, through the woods, to the situations at a distance from the beach.

The next town at which observations were made was Cannes. The position selected for testing the air of this district was the top of La Croix des Gardes, a small hill in the west bay, having an elevation of about 500 feet. The hill is situated quite outside the town, but it is surrounded on most sides by houses at no great distance. On the 10th of April, when there was a strong west wind, the air was very pure, there was only 1600 particles per c.c. noted. The air on this day was likely to be as pure as it will ever be at this situation, as it came with a fresh wind direct over the Esterel Mountains, and from a very thinly populated district. The lowest number observed here was much less than the lowest at Hyères, but at the latter place the wind on the days the tests were made never came from the least populated district, so that the observations taken at the two places do not admit of comparison. The observation taken at Cannes on the 12th show an entire change in the state of matters. The number on this day was very great, being as high as 150,000 per c.c. This was due to a change of wind, which blew direct from the town, and brought the products of combustion to the place of observation.

On the 13th, observations were made on the sea air at Cannes. The wind was from the south, that is inshore, but it was light. These observations on sea air can only be made when there is little

wind, otherwise the spray produced by the breaking waves introduces a disturbing element. The place selected for testing the sea air was in the west bay, at the outer end of the small iron pier. There were no waves breaking outside of the position of observation, and the number obtained would be correct for the air at the time. The number was very large, 10,000 per c.c., and when again tested on the beach to the lee of the breakers, which were high considering there was so little wind, the number was 12,000 per c.c. This increase on shore may have been due to particles originated by the waves. The very large number got at this situation would seem to suggest that the air had not come from far over the sea, but might have been land air which had circled round as the wind was light, or it might have been the air of the land breeze of the previous night returning as a sea breeze.

Very few favourable opportunities occurred for making observations on the air of Mentone. On the afternoon of the 25th of April tests were made on a hill about 1000 feet high, situated to the north-west of the town. When the observations began the wind was from the north, that is from the mountains, and there were 1200 particles per c.c. in it. A little later the wind changed and brought up the air of the town, when the number began to rise, and became as great as 7200, and was still rising when the observations were concluded.

On the morning of the 27th April I took a boat and pulled out to some distance seawards of the Mentone breakwater, and tested the air coming in from the sea. The number was 5000 per c.c. There was not wind enough to crest the waves. Later in the day the wind increased in force from the south-west, so that possibly the air examined in the morning may have been true sea air.

A number of observations were made at Bellagio on the Lake of Como. All these tests showed the air to be very full of dust for a mountainous district. But during the time of my visit the wind always blew from the south, that is from the inhabited parts of Italy; and, further, it was always light, so that local impurities were not swept away. The tests were made at an open window overlooking the lake, when the wind blew towards the shore, and at other times they were made on the top of the hill at the Villa Serbilloni.

The Baveno observations were made in similar weather to those of Bellagio, that is with little wind on most days, and the air damp and generally very thick, due to an accumulation of local impurities. The only occasion on which a low number was obtained here was on the afternoon of the 16th May. This pure air was found at some distance from Baveno, at the entrance to the Simplon Pass, above all the villages near the lake. The wind was light, but it descended from the mountains and was pure; owing however to the imperfect mixing of the hill and valley airs, the numbers varied a good deal. As will be seen from the table, they varied from 546 to 850 per c.c.

On the 18th of May the air was tested at Locarno, at the head of Lake Maggiore. The place selected for observing was on the hill-side above the pilgrimage church of Madonna del Sasso, about 600 feet above the lake. The wind was northerly, that is from the mountains, and the air was fairly pure, only 1300 per c.c. There was thunder at the time, and there had been heavy showers all day.

The Rigi Kulm Observations.

Being desirous of making some observations on the air at the top of mountains, the Rigi was selected as the most suitable for the purpose, on account of its elevation and isolated position. The convenience of being able to live at the top was another recommendation, as one would always be in a position to take advantage of any meteorological change which might take place. In all these respects perhaps Mont Pilatus would have been a better place for the purpose, as it is more isolated and is 1000 feet higher, but at the time of my visit the railway was not finished, and there was a difficulty in getting up apparatus and luggage sufficient for a visit of some days. I found the Rigi Kulm hotel a very convenient and comfortable place for observations of this kind. The hotel is so near the top of the hill, that on any change taking place one can be at the top in a minute or two and have a clear view all round, and the dust-counting apparatus can be placed in such a position that it tests the true passing air from whatever direction the wind may happen to blow at the time.

As the Rigi observations will probably be the most interesting, I have entered in the table the results of most of the tests, and later

on will have something further to say with regard to certain meteorological phenomena observed here. I arrived at the top of the mountain about midday on the 21st May. On the way up we entered a dense fog at about 2000 feet from the top. This fog continued all the way up, and at the top it was impossible to see more than 50 yards in front. I began work at 2 o'clock, selecting the top of the hill as the most suitable place for observing. The result was not satisfactory. The number of particles was found to vary greatly, going up to 1300 per c.c. and falling to under 500 in a short interval. This at once suggested local contamination, from which I was unsuccessful in freeing the observations, by change of position, as the thickness of the air prevented me seeing from what direction the polluted air might come; I had therefore to return to the hotel for information as to the direction of all possible sources of local pollution. I was then informed that the gas-work, engine-house, and other buildings connected with the hotel were situated on the slope of the hill to the windward, and within a few hundred yards of the place of observation. Having ascertained the direction of all possible sources of local pollution, a new situation was selected, well clear of all possible contamination, and a number of tests made. In this situation the number of particles was fairly constant, being 285 per c.c. when the testing began, and falling to 210 an hour later.

The morning of the 22nd opened bright and fine. The fog had cleared off the hill top, the clouds having sunk to a lower level and cleared away a good deal to the south; but to the north the whole country was covered with a nearly uniform layer of clouds, the upper surface of which was about 1000 feet below the top of the mountain. In the morning the number of particles was about 800. per c.c., at 10.30 A.M. the number fell to about 500, and in the afternoon the number was 1450 and gradually increased to 2300. During the afternoon observations an occasional cloud passed over the hill top.

As will be seen from the table, the number of particles on the varied greatly during the morning observations, the successive tests made within an hour varying from about 500 per c.c. to 1100 per c.c. This want of uniformity in the passing air may have been due to the strong wind blowing at the time, bringing imperfectly

mixed masses of the impure air of the valleys to the hill top. In the afternoon the wind fell a little, and the numbers were more constant, varying from 600 to 850.

The observations made on the 24th show the air on the morning of this day to have been much the same as it was the previous afternoon. The wind had fallen still further, and the number recorded between 10 A.M. and 11 A.M. varied but little, being from 617 to 685, showing a more uniform condition and a lower average than the previous day. At 4 P.M. the number had decreased to about 400, and by 5 P.M. the number tended to increase and become the same as it was in the morning.

The next day, the 25th, being the last day of my visit to the Rigi Kulm, I determined to make a number of tests on my way to Lucerne. Between 9 A.M. and 10 A.M. observations were made on the top of the mountain. The number observed was fairly constant, varying from 532 to 580. The wind was still blowing from a southerly direction, but not strong. On descending to Vitznau on the lake, I at once proceeded to make observations on the air at this level. The result at first somewhat astonished me, as the tests gave only about 600 per c.c. On examining the conditions, I noticed that the wind had greatly increased in force, and at this situation was blowing in strong gusts, and it did not come down the lake but came over the shoulder of the Rigi, and struck direct down on Vitznau, and spread out fan-like from the village as a centre. On each side of Vitznau the wind blew from the village, and on the lake in front it was off shore, so that the air tested at Vitznau had come from near the top of the mountain and would be much the same air as that tested in the morning at the top of the hill.

Leaving Vitznau I took steamer for Lucerne, and again tested the air, selecting for the purpose a position on the lake side about a mile up the lake and to the windward of the town. The number observed here was but slightly increased from what it was at Vitznau, being only about 650 per c.c. At this situation also, the air came direct from the mountains to the place of observation without local pollution, as it was driven down the lake by a strong wind, and came to the instrument from off the water, so that unless from a passing steamer there was no chance of pollution. The air was again tested in Lucerne at the open window of the hotel overlooking

a garden, and even here the number was small, but as the hotel is situated on the lake there was but little chance of pollution.

These Lucerne and Vitznau observations showed the air of this part of Switzerland to be remarkably pure. We must, however, remember that on this occasion the wind was strong, and that it came from the wide unpolluted tract of country covered by the mountains and valleys of the Alps. On the day following these tests the wind changed and blew from the north-east, and brought up the air from the inhabited parts of Switzerland. The tests made on this day show a marked increase in the number of particles. Though the wind had only recently changed, the pure air of the previous day had acquired a great increase in dust in its passage through the inhabited parts. The number on the afternoon of this day varied from 2160 to 3660 per c.c. in the open country near Lucerne.

The numbers obtained at all the Swiss stations, beginning with Locarno, are small, but the number of observations are too few to give anything like a true indication of the state of the air in that country. Still there are other reasons for supposing that the air of Switzerland is very free from dust. The vast tracts of mountainous country which surround it on most sides act as purifiers of the air, not only on account of the absence of population, and the time given for the dust to settle out of the air, but also by the pure upper air being mixed with the lower impure air, by the irregular currents and eddies formed by the wind in its passage across the mountains. It seems therefore possible that much of the transparency and brilliancy of the Swiss air may be due to its freedom from dust.

The Eiffel Tower Observations.

We have reason for supposing that the air at the top of mountains even such isolated ones as the Rigi, is not pure air, the same as one would get if they ascended to the same height in a balloon; but that along with the pure air there is mixed much of the air of the valleys driven up the slopes by the wind or carried during calm weather by the air heated by the sun on the mountain slopes. The amount of this lower air which may be carried by the wind to the upper level depends on the shape of the hill and other

conditions. If the hill is a long ridge, and the wind blowing at right angles to its length, the probability is the impure air of the valley will come almost unmixed with pure air to the hill top. Balloons might be used to carry the observer free from the pollution of the lower air, but they are evidently unsuitable for observations of this kind. The Eiffel Tower, however, looks as if it had been specially constructed for the purpose; only it does not go high enough and is not movable. The wind-driven air passes through the open framework of the tower, and the structure has no tendency to cause the lower air to rise and mix with the upper. Its position in a city, however, confines any investigation done on it, to the special conditions, which are only in a secondary way meteorological. Though not suitable for general meteorological work, the tower is evidently very suitable for investigating the vertical distribution of the impurities rising into the air over cities. As it is indeed the only place where satisfactory work of this kind can be carried on, I determined to return by Paris and if possible make some observations on the top of this tower. Owing to the kindness of M. Eiffel I was enabled to get access to the tower before it was opened to the public, M. Eiffel very kindly accompanying me to the first stage by the lift. From there I ascended to the top by the stairs, the upper lifts not being then finished.

The observations on the Eiffel Tower were made on the 29th May. The day was cloudy, with passing showers and strong southerly wind. On my way up the tower I stopped for a short time at the platform about 100 m. from the top, and tested the air at this elevation. The number at this height was 41,000 per c.c., showing the air to be very much polluted to a height of 200 m. I then ascended to the top, passing through the top platform and ascending some distance higher, and made the test on the small gallery just under the electric lamp, at about 300 m., or a little under 1000 feet from the ground. Tests were made at this elevation from 10.15 A.M. till 1 P.M.

My intention before starting was to make a series of tests at different elevations to try and find the rate at which the city impurities decreased with the height above the ground. A few observations on the top soon showed this plan to be impracticable, that with anything like the limited series of tests I would be able

to make it would be impossible to find any regular diminution at the different heights. I therefore confined my attention and gave most of my time to the variations taking place at the top, as a series of observations made there seemed to promise to give more information than a few taken at different heights.

It will be seen from the table that the numbers varied greatly from time to time, the changes taking place very quickly; indeed, it was often impossible to change the quantity of air used quick enough to meet the changing conditions. For instance, when the particles were few for a short time, they would suddenly increase to such a number that the quantity of dusty air which gave a convenient number of particles for counting gave in the next test a number so great they formed a fine fog, so dense it was impossible to count the particles. The two extremes which were counted with any degree of certainty were 104,000 per c.c. and 226 per c.c. The last and low number remained fairly constant during the taking of the ten tests of which the above is the average. This small number was obtained while a shower of rain was passing over the tower. As this rain was quite local, falling only over a small breadth of the city, the descending drops would strike down the impure city air and at the same time draw the pure upper air downwards. It seems probable that while this shower was passing, the top of the tower was in nearly pure air, probably of a level higher than its top.

An examination of the Eiffel Tower figures show that the impure air of the city must rise to a much greater height than 1000 feet. They also show that this air does not diffuse itself uniformly upwards, but rises in great masses, the rapid rise and fall in the numbers showing that the hot city air rises in columns through the purer air above, and that at considerable elevations over a city the air is of a very different character at places quite close to each other. It seems possible that a sensitive quick-acting thermometer might show these changes in the passing air by the rise and fall in the temperature. This want of homogeneity, apart from the dust and impurities, is one of the causes of the unsuitableness of the air near a city for astronomical observations.

On the way down from the top of the tower a halt was made at the lower platforms, and tests made at a height of 200 m. and at 115 m. As will be seen from the table, the results of these tests are of little

value, too few observations being obtained. After descending from the tower some tests were made of the lower air. These tests, owing to the kindness of M. Mascart, were made in the garden of the meteorological office in the Rue de l'Université. The situation is not far from the tower, and the air at this place would be much the same as at the base of the tower. The number at the low level was very great when the tests were made. The number, however, varies greatly at low levels near the sources of pollution, owing to local circumstances, and the number varies greatly from time to time. On this occasion the number varied from 210,000 to 134,000 per c.c. In the same situation in March the number was much lower, being 92,000, the difference being probably due to atmospheric conditions. In March the air was much dryer.

London.

The only observations of interest made in London were taken on the 3rd of June. The day was bright and dry, with a fresh westerly wind. Observations were made at the window of an hotel in Victoria Street, and also in a garden on the bank of the Thames a little below Battersea Bridge. To this latter situation the air came direct from Battersea Park uncontaminated by immediate local pollution. It will be seen from the table that the numbers for Battersea Park varied from 48,000 to 116,000, the number varying a good deal with the velocity of the wind. It would be interesting to know what the number is at this situation when the air is damp and heavy and but little wind blowing. As no such day occurred during my visit, there was no opportunity of testing this point.

Kingairloch.

The next observations entered in the table were taken at Kingairloch in July. Kingairloch is situated in Argyleshire, in the wilds of the West Highlands, amidst the mountains on the north-west side of Loch Linnhe, nearly opposite Port Appin. At this situation the air is very free from local pollution, there being only a very few houses for many square miles. The air at this situation was generally very pure, but the amount of dust was occasionally found to vary considerably at short intervals. This was particularly the case when the wind blew across the valley in which the

observations were made. This rapid variation in the numbers was no doubt due to the cross currents mixing masses of upper and lower air, so that sometimes one and sometimes the other passed the instrument. This seems to be the probable explanation of the variations sometimes noticed here, as they were too small to be due to local impurities.

The table shows that the purity of the air, even at this isolated situation, varied from day to day, being as high as 4000 particles per c.c. on the 4th and as low as 205 per c.c. on the 6th. The higher numbers were generally got with southerly winds and the lower with northerly ones. This relation of direction of wind to number of particles would seem to indicate that the higher numbers were due to pollution from the inhabited parts of the country, as all to the east and south of this situation is densely populated, whilst in the other directions there are but few dwellings. It is true that the distance to the densely populated parts is very considerable, yet from the extreme slowness with which these fine particles settle, and the short time required for the wind to bring them to the place of observation, there seems to be a possibility of their being carried as far as this. When we remember the number observed on the top of Finouillet when the wind blew direct from Toulon, we can understand how it is possible for the dust of the densely populated parts of the lowlands being brought to the wilds of the Highlands. It may be remarked that an examination of the weather charts for these dates shows that much reliance cannot be placed on this conclusion, as the winds observed may have been and sometimes were quite local, and the distribution of pressure was such that the winds were light and the direction of the general circulation frequently doubtful.

Ben Nevis.

The Council of the Scottish Meteorological Society having proposed to acquire dust-counting apparatus for the observatory on the top of Ben Nevis, for the purpose of carrying on a regular series of observations on the dust in our atmosphere at that situation, it became necessary for me to make an ascent of the mountain for the purpose of seeing what arrangements would be required for the carrying out of this investigation under the conditions existing

there. My visit was made on the 1st August under fairly favourable conditions of weather. The morning was dark and cloudy, with detached patches of mist on the hill-sides. Fortunately as the day advanced the mist gradually rose, and as I ascended the mountain it cleared away and at last was all gone by the time the top was reached. The air was fairly clear, as we could see Schiehallion quite distinctly on the one side and the hills of Skye on the other, the whole horizon within that range being clearly visible all round. The day, however, was favourable for transparency, as the sky was clouded all over and the sun did not shine on and illuminate the little haze there was in the air. On testing the air the number of particles counted by Mr Omond and myself was 335 per c.c. at 1 P.M. and 473 at 3 P.M.

Alford.

Observations were made from the 5th to the 15th September at Alford, a small village in the valley of the Don in Aberdeenshire. These tests were made near the Manse of Alford, which is situated at a distance of about 2 miles to the west of the village. There are but few houses near, so that the air would be fairly free from local pollution. An examination of the figures given in the table for this situation shows that the air here was not quite so pure as at Kingairloch. This might possibly be due to there being more houses in this locality than in the neighbourhood of Kingairloch, but part was also due to the weather. While these tests were being made the wind was generally very light, and frequently blew from the east and south-east, that is from the polluted districts, and it rarely came from the mountains, so that probably the air was a good deal polluted; and, moreover, owing to the wind being generally light, the air was stagnant and local pollutions would tend to accumulate. It is probable that the numbers given for this situation are too high for an average for the district, as the weather at the time was close, dull, and thick, with a marked absence of the usual clear crisp Aberdeenshire air of September.

On the morning of the 9th the wind blew from the south-west, that is from an unpolluted direction; it was therefore determined that an ascent of Callievar should be made that day, for the purpose of testing the air at the top. Callievar is a somewhat isolated

mountain, situated at a distance of five or six miles to the west of Alford. It rises in easy slopes from the east and south, though rather steep on the north and west, and attains a height of 1747 feet. The early morning of the 9th had been very thick, but it soon began to clear. On testing the air at 9 A.M., before beginning the ascent, it was found to have 3000 particles per c.c.; on arriving at the top of the hill the number was found to be much lower. At midday there were only 262 per c.c. by the first test and 336 by the second, which was taken shortly afterwards, each of these numbers being as usual the average of ten tests. On again testing the air at 2 P.M., two sets of tests gave the same average, namely, 475 particles per c.c. This gradual increase was probably due to local contamination. When the first tests were made the wind was south-west, and came over an uninhabited district, whereas by the time the last tests were made the wind had veered to the westward, and brought the smoke of the dwellings in the Don valley towards the hill top. The view from the hill on this day was fairly clear, the Cairngorms and Lochnagar being quite distinct, but seen through a good deal of haze, which was illuminated by a bright sun, shining in a cloudless sky. On returning to the foot of the hill and again testing the lower air it was found to have 900 particles per c.c. It seems therefore probable that the lower air at this situation is considerably contaminated by local causes.

Dumfries.

The last series of observations entered in the table were made at Isle, near Dumfries. The site of these observations is quite in the country, at a distance of six miles, up the Nith valley, from Dumfries; it is not near any village, but is surrounded in all directions by farm and other houses. The site being on the banks of a river, the air at it will have the peculiarities belonging to such situations, such as greater humidity, less air circulation, and lower temperature at nights than at more exposed situations.

An examination of these Dumfries figures shows the air at this situation to have been remarkably impure for a country district. It was probably much more impure than it generally is, owing to certain meteorological conditions prevailing during the time these tests were being made. The table shows that the air was very pure

on the 30th of October, the number being as low as 235 per c.c., and it was fairly low on one or two other days, but on many occasions the number was over 5000 per c.c. The weather accompanying this impure atmospheric condition was true November weather, dull, thick, and damp, with only an occasional fine day.

On the Pollution of the Atmosphere by Artificial Causes.

On looking over the numbers given in the table one is struck by the amount of dusty pollution introduced into our atmosphere by human means; and one cannot help feeling that the earth's atmosphere is profoundly modified by the vast quantities of dust it holds in suspension, much of which owes its origin to the presence of human beings. When we ascend into the higher atmosphere, or go to uninhabited districts, the quantity of dust in the atmosphere is comparatively small, but whenever we approach human habitations the quantity increases, and near cities the air is laden with these floating particles. At the top of the Rigi, and in the wilds of Argyleshire, air was tested which had only a little over two hundred particles per c.c. On the top of Ben Nevis, on Callievar, and in Dumfriesshire, the number was occasionally only about three hundred, but when we get near villages, the number goes up to thousands, and in cities to hundreds of thousands.

Though the number of particles in the atmosphere increases enormously as we approach the centres of human habitations, yet when we look more closely at the numbers in the table, we find that the increase, though great, is not by any means in proportion to the increase of the sources of pollution. A small village or town may make the passing air nearly as polluted as a large city. Now why should the pollution not be in direct proportion to the number of fires and other sources of contamination. Is it possible that when the number of particles becomes very great, so that they are very close together, that many may unite and form one? so that what we count as one, in city air, may really be a vast number adhering together. As yet we have no information as to whether these very fine dust particles ever do attract and adhere to each other or not. There is, however, an evident reason why the particles do not increase in number in direct proportion to the amount of the pollution, which is this,—with the increase in the size of the city there is not

only an increase in the number of particles in the air, but there is also an increase in the depth of the polluted stratum of air, over the city. The Eiffel Tower tests showed that the polluted air of Paris rose to a height of more than 1000 feet. Probably on the day the tests were made it may have extended to many times that height, as extremely polluted air came to the top of the tower from time to time. The larger the city the greater will be the mass of heated air rising over it, and the greater its ascending power, so that the depth of polluted air leaving a city must be very great, while that leaving a small town will be much less. So that when we add to our knowledge of the number of particles, the greater depth of the polluted stratum of air over cities, we are better able to understand why the air of cities is not more impure than the tests show it to be.

When the Eiffel Tower tests were made the air was fairly dry, which favoured the ascension of the heated air. When, however, the air is damp, it seems probable that the products of combustion will become loaded with condensed moisture, and this will counteract the ascending tendency of the heated air, and the thickened air during this weather will probably keep lower down and give rise to the dull thick town atmosphere of damp weather, and to town fogs when the wind falls, especially if the condensation is intensified by the cold produced by radiation.

It may here be asked, Is 200 particles per c.c. the lower limit of the purity of our atmosphere, or is there such a thing as air absolutely free from dust? The air at extreme elevations never has, and probably never will be tested; but nature has provided a test which shows that even at very great heights dust is present. Where-ever a cloud forms we know there is dust, and as clouds form at great elevations we may conclude that dust is present at even these great heights. It has been shown in a previous paper that condensation may take place in dust-free air; but while this is so, yet the conditions under which this is possible are such as never happen in nature, because before it can take place the air requires to be greatly supersaturated, and be put into rapid eddying movements, or receive some violent shock, a condition of things never likely to happen in the upper strata of the atmosphere. And further, if condensation did take place, it would not take the cloudy form but the rainy one; that is, the centres of condensation would be few

and widely separated, so that each particle would be large and fall rapidly as rain. At present therefore all the evidence points to the conclusion that dust is present in our atmosphere at whatever elevation clouds form.

Now though this conclusion may be true, so that on the one hand we may have evidence that at very great heights in our atmosphere there may be enough dust to form clouds, and that while experiment shows there may be only 200 particles per c.c. in the air, we are yet without any guide as to what is the number of particles in pure unpolluted air. The air at tops of mountains, and even at great heights away from mountains, may be polluted by artificial causes. The dust of the lower air will be carried by ascending currents to great heights, and much that is present in what we have called pure air may be of terrestrial origin. But even supposing the lower air never carried dust to great heights, yet there will be dust there, due to the disintegration of the vast quantities of meteoric matter, which is daily showered down into our atmosphere, where it becomes highly heated and dissipated, thus forming fine meteoric dust. What the number of particles of this meteoric dust may be, we cannot say, nor can we tell what its proportion to terrestrial dust may be at the different elevations, but it seems probable that the meteoric dust will vary in quantity from time to time, with the earth's position in space.

Dust and Atmospheric Transparency.

When I made my last communication to the Society on atmospheric dust,* reference was made to the close relation which was observed between the transparency of the air and the number of particles of dust in it, when the tests were made at Colmonell. At the same time it was pointed out that this relation was likely to hold good only under certain conditions, that the amount of vapour in the air, or rather the humidity or degree of saturation, would be likely to modify the effect of the dust. My reason for qualifying the Colmonell conclusion was that it had been shown in a previous paper † that there is an affinity between the dust in our atmosphere and water vapour, and that some kinds of dust begin to condense

* *Proc. Roy. Soc. Edin.*, vol. xvi. p. 160.

† *Trans. Roy. Soc. Edin.*, vol. xxx. part i.

moisture before the air is cooled to the dew-point, that is, condense vapour in unsaturated air.

It is evident, therefore, that if the ordinary dust of our atmosphere has an affinity for vapour, and causes condensation to begin while the humidity of the air is some distance from the dew-point, that the condensed moisture will increase the size of the particles, and by so doing cause a decrease in the transparency of the air, under conditions which at present are not understood. It will be therefore necessary for us to bring a considerable amount of evidence to bear on this point, as the opinion generally held is that though condensation may be caused to take place in unsaturated air by the artificial introduction into it of certain vapours and gases, yet condensation does not as a rule begin in our atmosphere till the air is saturated.

The question then comes to be, Is there any evidence to show that condensation really does take place on the ordinary atmospheric dust, before the air is saturated, and if so at what stage of relative humidity does it begin to take place? The dust-counting apparatus evidently places in our hands a method of investigating this point; indeed, without it, it would be nearly hopeless to attempt its solution. During the investigation this is one of the points to which attention was closely directed, and the conclusion come to, from all the evidence that has been collected so far, is that we have been far too much in the habit of supposing that condensation can only begin just at or immediately before saturation is reached. This investigation distinctly points to condensation beginning long before saturation is attained, and while the air is still dry enough to give a difference of some degrees between the wet and dry bulb thermometers.

In order to get the necessary information to enable me to come to a conclusion on this point, it was customary to enter in the note-book, along with the number of particles observed, as many of the meteorological conditions as possible. Amongst other things noted were the direction of the wind, the temperature, the humidity, and the clearness of the air. These have been entered in the table, given with this paper. As already stated, the records of humidity and clearness are not very satisfactory, owing to the want of proper screens for taking the wet and dry bulb observation at the different stations and for the want of some satisfactory method of measuring

the transparency of the air, especially when one is constantly changing the position where the observations were made, and where the visibility of hills at different distances cannot be noted. Though these observations do lack accuracy, yet they point in a rough but perfectly distinct way to the conclusion that it is not necessary for the air to be saturated in order that condensation may take place; they rather indicate that at almost all states of humidity the dust has some moisture condensed on it, and that as the humidity increases the amount of deposited moisture increases with it, and increases at a rate much quicker than the humidity, especially when the air is approaching the point of saturation.

It may be remembered that in the Colmonell observations it was noticed that the transparency of the air depended on the number of dust particles in it; with 9000 particles per c.c. the air was extremely thick, with 5000 it was thick, and was clear only when the number fell below 1000. The Colmonell observations were made in winter, while the air was damp, and its humidity did not vary enough to have a great influence on the transparency.

To illustrate the effect of humidity, let us turn to the observations recorded in this paper. Taking the Hyères observations first, it will be at once seen that the humidity has a powerful modifying influence on the effect which the number of particles has on the transparency of the atmosphere. It is true that owing to local pollution the numbers observed at this station may be too high, and only correct for the air near the ground. We shall take only the Finouillet and La Plage observations as of any use, the others being too polluted from local causes. It will be seen from these that air with even more than 4000 particles per c.c. may be very clear and transparent if it is very dry—with that number of dust particles and a depression of the wet bulb of 10 degrees or more, the air was very transparent, as on the 21st March and 3rd April. It may be as well to note here that the observations taken on the 4th of April, when the number of particles was very great, are of no value for our present purpose, as on this day the wind brought the smoke of Toulon direct to the top of Finouillet, and while the air generally was clear, yet in the direction of Toulon it was thick.

The effect of humidity is very clearly seen in the observations taken at Cannes. There was no very great difference in the number

of particles on the mornings of the 10th and 11th April—but the air on the 10th was very clear, owing to it being very dry, the wet bulb being depressed 13°; while the morning of the 11th was thick, owing to a great increase in the humidity, the wet bulb on this occasion being depressed only 2°.

The Bellagio and Baveno observations show the same result, but not so satisfactorily. Though every precaution was taken to test the air where it was free from local pollution, yet it is evident this was not possible under the conditions at these stations.

Coming now to the Rigi observations, it will be noticed that the air was thickest on the 21st, when it contained the fewest particles. This thickness was owing to humidity, a good deal of moisture being deposited on the dust. It will be noticed that there was a depression of 1°·5 in the wet bulb at the time, showing that the air was not saturated. As this was the first occasion on which I had made observations on this dense form of condensation, I carefully noted the state of the air with regard to dryness. The depression of the wet bulb given in the table may not be quite correct, though taken as carefully as possible under the conditions. It may possibly be too great, owing to the air having been heated locally. It was observed that all objects in the open air were quite dry, not only stones which might have been kept dry by heat communicated from beneath, but all surfaces, such as wooden fences, &c. The only exception to this was the grass, which carried drops of moisture at the tips of its blades, but this came from within the plant, there being no indication of the other parts of the blades being wetted by fog.

It is, however, very evident that though all exposed surfaces were dry on this occasion, it does not necessarily follow that the air was not saturated. The reason why the fog particles did not wet the exposed surfaces may have been that these surfaces were all heated by radiation, and the fog particles would be evaporated in the hot air in contact with the exposed surfaces, as radiation can undoubtedly penetrate a great depth of fog. I regret that no observations were made in this direction.* It may be noticed in passing, that the Swiss Meteorological Report for the Rigi Kulm shows the depression of the wet bulb at one o'clock on the 25th

* It has, however, been confirmed by later observations.

to have been 0°·9 C., or almost exactly the same as found by me. But as the thermometer at this situation is exposed in a metal box, it also would be exposed to radiation and slight heating. As there was a considerable amount of light at the time, there would be but little fog overhead. It seems therefore probable that radiant heat would penetrate and heat all surfaces and keep them dry, and heat the air in contact with them. The wet bulb depression recorded was therefore probably too high.

On the morning of the 22nd there were far more particles in the air than on the 21st, but the air was clear owing to an increase in the dryness, the wet bulb being now depressed 6°. As the day advanced the air got hazy, owing to an increase in both dust and moisture. On the 23rd the air was very clear, both dust and moisture having decreased. The dust and moisture remained much the same till the close of the observations, and the air retained its clearness to the end. So clear was it that occasionally Hochgerrach was visible, a condition of the atmosphere which, to those who know the Rigi, will be a good indication of the transparency.

At Lucerne on the 25th the dust and moisture were both very low and the air was very clear. On the 26th the dust increased considerably, while the humidity remained nearly the same, and the air was hazy.

No conclusions can be drawn on this point either from the Eiffel Tower or the London observations.

Passing over the tests of city air, we shall now enter more into detail of the Kingairloch observations, as the air at that situation was less polluted, and the observations are better suited than the previous ones for showing the effect of dust and of humidity on the transparency of the atmosphere. First let us consider the effect which the number of particles has on the transparency of the air, and see if the conclusion contained in the previous paper is confirmed by these later observations. This conclusion was that the greater the number of particles the thicker was the air. For this purpose, as already explained, we must select observations made on days when the humidity was the same. Taking those days on which the wet bulb was depressed 4°, namely, on the 6th, 10th, and 15th of the month, and arranging them in the order of the number of particles, thus—

Number of Particles.	Date.	State of Air.
550	15th, at 11.30 A.M.	Clear.
814	10th, ,,	Medium.
1000	6th, at 10.30 A.M.	Thick.
1900	15th, at 2 P.M.	Thick.

It will be seen that with a humidity corresponding to a depression of 4°, that if there were 550 particles the air was clear, and that as the number of particles increased the transparency decreased. Again, take the 12th and 13th; on both of these days the humidity was the same, namely, 5°, but the one was clear which had only 710 particles in it, while the other was thick owing to there being 3000 particles present. Again, there was the same humidity on the morning of the 5th as at 1.30 P.M. on the 6th, but on the 6th the air was clear owing to there being only about 300 particles in it, while on the previous day it was thick owing to the presence of more than four times that number.

These figures all tend to confirm the conclusion previously arrived at from the Colmonell observations, namely, that the transparency of the air depends on the number of particles of dust in it. We shall now see how far the Kingairloch observations support the other conclusion, that the amount of moisture in the air has also an effect by increasing the size of the particles, and so reducing the transparency. We shall use those Kingairloch figures and see if this affinity of the dust for moisture has practically any effect on the transparency of the atmosphere.

When the observations began on the 3rd of July there was a good deal of dust in the air, about 3000 per c.c. This degree of impurity continued next day, but fell to about one-half on the 5th and to one-third on the morning of the 6th. Now, though the dust gradually decreased during these days, it will be observed that the thickness gradually increased from a haze on the 3rd to thick on the 6th. The reason for this decrease in the transparency was the increase in the humidity which took place with the decrease in the dust, the humidity falling from a depression of 13° on the 3rd to 4° on the 6th. Here the decreased transparency produced by increase in size due to humidity more than counteracted the effect of the decrease in numbers, showing how powerful the influence of humidity is, even when the humidity is some degrees from saturation.

This dependence of transparency on humidity may be illustrated by these observations in another way. Taking days on which the number of particles was nearly the same, but on which the humidity was different. On the 5th, with 1500 particles and a wet bulb depression of 6°, the air was thick; with about the same number of particles on the 8th, but with a depression of 9°, the air had a medium clearness; on the morning of the 10th, with 814 and a depression of only 4°, the air was medium clear; whereas at 1.30 P.M. on the 16th it was very clear, with the same amount of dust, but a depression of 7°. With 500 particles and a depression of 4°, the air was clear on the 15th, but very clear on the 17th with the same number of particles but 8° depression. It will be noticed that when there was a depression of only 4° the air was thick, unless there was less than 1000 particles per c.c., and that if the numbers increased and the air was at all clear there was also an increase in the dryness.

It will be observed that the number obtained on the morning of the 9th does not agree very well with the others. The number on this morning was high and the dryness not very great, and yet the air had a medium clearness. We, however, leave the figures in the table, and the only explanation we can offer is that this number was local and due to an almost entire absence of wind at the time, though it must be admitted that the numbers are rather constant to be explained in this way.

The Alford observations point to the same conclusions. I must, however, state here that the figures entered in the columns headed temperature and humidity were not obtained by myself at the place and hour of observation, but are from the weather report of the Scottish Meterological Society's Station at Logie Coldstone, which has been kindly supplied to me by Dr Buchan. Further, the temperatures are the maximum temperatures for the day, and the humidities are the maximum humidities for the day calculated from the morning and evening readings considered with reference to the maximum temperature. The temperatures will therefore be higher than if they had been taken at the time the dust tests were made, and the wet bulb depression will also be much greater. We can therefore only compare these Alford tests amongst themselves. It is, however, unnecessary here to enter into a detailed

examination of the figures, the effects of both dust and humidity being evident.

The most interesting of the Alford tests are those made on the 16th of September. On the morning of that day the air was medium clear, with bright sunshine, and the number of particles was 1125 per c.c. As the day advanced the air thickened in a very remarkable way; from having a medium clearness in the morning it gradually thickened and lost its transparency as the day advanced, and in the afternoon it was extremely thick, but clear overhead and free from clouds. When the air was tested at 5.30 P.M. the dust particles were found to have increased to a higher number than had been observed at this station—the number was 5700 per c.c. The great increase in the thickness of the air which took place during the day was evidently due greatly to this increase in the amount of dust. Increased humidity may possibly have had some effect, but probably it was not the chief cause, as the wet bulb was only depressed 1°·5 more at 9 A.M. than at 9 P.M.

An examination of the Dumfries series of observations bears out the conclusions we have come to from the other figures. The relation between the transparency and the dust and humidity is evident, but the figures obtained at the last situation are not so clear on this point, probably owing to the air not being so free from local pollution. The Dumfries observations might have been omitted if it were not that they give us information on some other points. It will be observed that the numbers at Dumfries were generally very high, often extremely so, for a thinly populated country district. An examination of the figures in the column headed wind, shows us that these very high numbers generally occurred when the wind was slight or variable, as on the 13th, 14th, 16th, 18th, and 19th of November. The increase in the amount does not seem to have been entirely due to the direction of the wind bringing the air from impure sources, as the lower current as well as the general upper circulation varied on these days from south by west to nearly north. These large numbers may therefore have been greatly due to an accumulation of purely local impurities, as it will be seen that whenever the air was very impure it always got much purer whenever the wind increased. These Dumfries observations are evidently of less value than the Kingairloch ones for investigating the relation

between the transparency and the dust and humidity, because the air at the former place being polluted by local causes, it is probable there would be a want of uniformity in the air at different points near each other, and in the air from time to time. This was confirmed by the observations. The numbers varied a good deal at short intervals. The effect of the wind in reducing the number of particles may be noticed also in the Kingairloch and Alford observations.

There is a point of some interest brought out by a comparison of the Dumfries and Kingairloch observations to which it may be worth while directing attention. It is, that while the transparency of the air depends on the amount of dust, and on the humidity, or degree of saturation of the air, it would appear that the thickening effect of the humidity depends on the temperature, that is on the tension of the vapour. For instance, a humidity which gives a depression of 4° at a temperature of 60° has a much greater thickening effect than the same depression at 50°. This might have been anticipated, but it was only while the Dumfries tests were being made that it was thought that the air was clearer for the same humidity than it was at Kingairloch. An examination of the figures in the table supports this conclusion. For instance, take the following examples when the air had about the same amount of dust and about the same depression of the wet bulb.

Date.	Number.	Humidity.	Temperature.	Transparency.
5th July	1500	6	64	Thick.
6th July	1000	4	61	Thick.
6th Nov.	1360	4·6	50	Clear.
29th Oct.	1200	4·6	50·5	Medium.

Compare the above observations when the dust and the wet bulb depression were similar. In July, when the temperature was over 60°, the air was thick, whereas in October and November, when the temperature was 10° lower, the air was clear or medium. The other figures mostly point in the same direction. The air observed on Ben Nevis would probably not have been nearly so clear if its temperature had not been low.

This result I have said might have been anticipated, because at the higher temperature with the same wet bulb depression the vapour tension will be considerably greater than at the lower, and each particle will attach to itself a greater amount of water. These

conclusions, however, require many more, and much more carefully conducted experiments to prove them satisfactorily. The objections to the Dumfries observations are many. First, the situation is low, and the dust would tend to collect there to an undue amount. Second, the humidity is too high, owing to this station being near water, so that while the dust-counter and the thermometers gave the condition of the air in the valley, the tests for transparency were necessarily made through a higher stratum of air, in which both dust and humidity would probably be less. Then again, the difficulty of estimating transparency, and also in comparing these estimates made at different stations. The different hills visible at each station should have been graduated off, and used as a scale of transparency. This, however, was not done till the last observations were made.

Though these results should be received with caution, yet I think it will be admitted that they clearly point to an interesting line of enquiry, and encourage us to push on our investigations into the dust in the atmosphere. I may remark that it is advisable not to make comparisons of these tests when the wet bulb is depressed only about 1°, because when the saturation is so great, accuracy cannot be attained, as local conditions, such as wet trees, the exposure of the bulbs, &c., may cause an error equal to the whole depression.

The Dumfries observations show an interesting relation between the weather and the amount of dust in the air. During the whole time the weather remained dull and thick, the air was highly charged with dust. Another important point was that when the clouds cleared away, dense fogs formed, owing probably to the great radiating power of the air, due to the great amount of dust in it. It will also be noticed that on the fine days the air had generally little dust in it.

The conclusion forced upon us by a consideration of all these observations is that the dust in our atmosphere condenses vapour when the air is far from being saturated. It seems probable that in all states of humidity the atmospheric dust has some moisture attached to it, and that as the humidity increases the load of moisture also increases; and further, all observation seems to point to this form of condensation producing even a very thick condition of the atmosphere before the air is quite saturated. The observa-

tions made on the Rigi, when the mountain top was covered with cloud, points to the formation of even this dense form of condensation in unsaturated air. It must be admitted that the Rigi observations of the humidity on this day are not very reliable, and the subject might be dismissed were it not that there are many observations made by other observers, on Ben Nevis and elsewhere, which point to the same conclusion.

There is another point to which I can only call attention at present, as it has not yet been investigated. It has reference to the effect of the direction of the wind on the thickness of the air. It has been sometimes thought, while these tests were being made, that with certain directions of wind the air was unduly thick for the number of particles and the humidity. This would imply that the dust particles brought by certain directions of wind were larger or more hygroscopic than those brought from other directions. This seems possible, as it is the winds from inhabited districts that are suspected of being unduly thick. As these bring the sulphur products formed in burning coal, it seems possible they may be charged with vapour condensing particles. This point, however, is one that requires further investigation.

Apparatus for Testing the Condensing Power of Dust.

While on the subject of the condensing power of dust, or the affinity of atmospheric dust for vapour, I may here refer to some experiments made a year or two ago on this point. The principle I then tried to develop was to collect the atmospheric dust on glass plates, and test if water vapour condensed on the deposited dust in unsaturated air. The following was the plan adopted of carrying out this idea. First as to the method of collecting the dust. This was done by placing glass plates inside a thermometer screen, a room, or wherever it was desired to get the dust to be tested. These plates were sometimes placed horizontally, and the dust allowed to fall on them. Another plan of collecting the dust was to deposit it on the glass plates by the method used in my thermic filter. If it was desired to collect the dust inside a room by this method, the glass plate was placed vertically and in close contact with one of the panes of glass in the window. The plate was kept in its place by means of a little india-rubber solution. As this plate was colder

than the air in the room the dust was deposited upon it. If it was desired to collect the dust in the outer air, the plate of glass was attached to the outside of the window pane, but kept at a distance of two or three millimetres from it. It was held in its place by the india-rubber solution, four pieces of sheet india-rubber being fixed at the corners to keep the plate away from the window glass. By this arrangement the outside air circulated in the space between the outside plate and the window, and as this air got heated on the window pane it was warmer than the plate and deposited its dust on its cold surface. It may be mentioned that the plates used for collecting the dust were small pieces of glass mirror about 10 cm. square. Mirrors were used, as the condensed vapour is more easily detected on them than on clear glass.

Having collected the dust in either of these ways, its condensing power was tested in the following manner. A small cell was prepared about 4 cm. square and 1·5 cm. deep, and open at the top. The top edge of the cell was covered with a thickness of india-rubber. The dusty plate was placed so as to form a cover to the cell, being held down by means of spring catches; the india-rubber enabled the plate to make a water-tight joint with the cell. The cell was provided with two pipes, one for taking in water and the other for taking away the overflow. A thermometer was fixed with its bulb occupying the centre of the cell; and further the cell was provided with a stirrer. The whole arrangement was almost exactly the same as a Dines' hygrometer. The dusty plate, before it was put in its position, had the dust carefully cleaned off one half of it, so that when put in its place one half of the glass covering the cell was dusty and the other half clean. Cold water was then run through the cell and the temperature of the plate gradually lowered, the plate meanwhile being closely watched to see when condensation began on the different halves, and the temperature noted when it began on the dusty half and when it began on the clean part, and the difference, if any, noted. In this way a measure of the condensing power of the dust was obtained, as the difference between these two temperatures gives the temperature above the dew-point at which the dust condensed vapour.

It will be noticed that the plates for collecting the dust were much larger than the cell. By this means the cooled surface over

the cell was surrounded by an uncooled part of the plate, and any change in the appearance of the cooled dust could be easily detected by contrast with the surrounding area.

The first thing to be done was to test the working of the apparatus, and see how it acted with dusts of known composition and known condensing power. For this purpose smoke of burning magnesium, gunpowder, and sodium were used—the first on account of its small affinity for water vapour, the second for its condensing power. It is well known that the smoke from gunpowder is far more dense in damp than in dry weather, the difference being due no doubt to the condensation which takes place on the smoke when the air is damp. While sodium smoke was selected on account of its great affinity for water. The smoke of these substances was produced by burning a little of them in an enclosed space, the test plates were placed in the enclosure, and the smoke allowed to fall on them. The mirrors with the deposited dusts were then tested, and their condensing powers measured, with the following result:—Magnesia was found to condense at almost exactly the same temperature as the glass, but gunpowder smoke began to show signs of condensing at a temperature 5° above the dew-point; while the soda condensed vapour from air at a temperature 17° above its dew-point. In making these tests it is necessary that the air in the room, where the testing is done, be very dry, otherwise the beginning of the condensation on the dust cannot be detected, because the dust surrounding the cooled surface already has some moisture condensed on it. To overcome this difficulty it was customary to heat and dry the plates before testing them.

The different kinds of dust, when tested in this way, having thus shown a distinct difference in their affinities for vapour, tests were now made of the dust collected from the atmosphere. It is unnecessary to go into the detail of these experiments, as although they distinctly point to atmospheric dust having an affinity for water vapour, and this affinity seemed to vary with the dust, yet the method of testing is not very accurate. As the condensation begins by imperceptible degrees, it is very much a question of quickness of perception, and carefulness of working, that determines when the first appearance of condensation will be detected; and even the first appearance is not the real beginning, but only the state at which it

has become visible to the observer. It may, however, be mentioned that dust collected in a smoking-room showed a decidedly greater condensing power than that from the outer air. Of ten tests made with dust from the smoking-room, the dust that had the least condensing power showed condensation at a temperature of 2°·2 above the dew-point, whilst the most powerfully condensing dust condensed at 4°·5 above the dew-point, while the mean of the ten gave 3° above the dew-point. Of the ten tests of the dust from the outer air, the lowest showed a condensing point of 1°·8 above the dew-point, and the highest of 3°·2, and a mean of 2°·3. It should be mentioned that the depth of the deposit did not seem to have any influence on the condensing power of the dust, as the amount of dust that collected in one day gave about the same effect as that collected in thirteen days.

This condensing power of dust would seem to explain why it is that the glass in picture frames, and other places, frequently looks damp when the air is not saturated. The same damp deposit may be easily seen on windows during cold weather, particularly if they have not been cleaned for some days. The damp-looking deposit can be easily detected by cleaning a small part of the pane. The cleaned part will remain undewed, while the surface surrounding it will be damp, and be greasy when rubbed. I need not say that a certain degree of humidity in the air is necessary for seeing this clearly, as the glass must be cooled by the outer air to near the dew-point of the air in the room. Again, this condensing power of dust may in part explain the reason why it is so necessary to keep electrical apparatus free from dust, if we wish the insulation to be good. The damp collected by the dust will decrease the insulating power of the glass or other insulator.

Dust and Condensation.

We have seen that these observations all point to the conclusion that moisture is deposited on the particles of atmospheric dust in air which is not saturated. This condensation seems to take place while the air is comparatively dry, and the amount deposited increases with the humidity. The deposit which takes place under these conditions will probably be caused by the affinity—chemical or surface—of the particles for vapour, while true cloud condensation

seems only to begin when the air is near the point of saturation. There seems no definite degree of humidity necessary for the first form of condensation to begin, or if there is, it begins while the air is very dry, and by such imperceptible degrees, it has not been noticed. There is no hard and fast line between what we call clear air and thick haze. The clearest air has some haze, and the effect of increasing humidity is to increase the thickness of the air. But from clear air up to cloudy condensation there is no real difference in kind, only in the amount, of the thickening. But when cloudy condensation begins, a real change in the nature of the condensation takes place. The dust particles have now no tendency to condense the vapour; each particle seems to have got its affinity satisfied, and true condensation begins, owing to the tendency of the saturated vapour to condense. The affinity of the particles has now little influence, but the *size* of the different particles has; the larger ones getting the most moisture deposited on them. All the particles at this stage seem to cease to attract the vapour, and the vapour is no longer deposited on the whole number of particles; but a comparatively small proportion, and these the larger particles, receive the whole of the condensing molecules. This seems to be the reason why at about saturation a change occurs in the appearance of the condensation, a sudden thickening of the air takes place along with an increased light reflecting power.

It might be thought that if this incipient condensation, due to affinity, took place before the true condensation, then there ought to be some evidence of it in the every day phenomena of nature. For instance, does the upper moving front of a cumulus cloud show this incipient stage of condensation? At first sight we might expect it would. I do not think, however, that it has been noticed, possibly owing to the conditions necessary for seeing it not being easily obtained. We cannot expect to see it at the upper surface after the cloud is formed, the transition at that surface from saturated to dry air is far too sudden. If we are to see it, in the formation of a cumulus cloud, we must look for it just before the cloud begins to show, in the clear and rising air, where it is possible we may find a thickening taking place before, and at a lower level, than that at which the true cloud condensation begins. This as yet has not been observed, but in other conditions the

gradual thickening has been frequently seen. At the Italian lakes, on many occasions, when the air was damp and still, there has been noticed in close proximity every stage of condensation, not divided by the hard line usually observed, but where it was impossible to say where the thick air ended and the cloudy began. Again, this gradual change can often be seen in the sky overhead. With approaching change of weather, the sky is often seen to change by imperceptible degrees from perfect transparency to "thick," and then to cloud. It is very doubtful if we are entitled to expect to find any well defined haze in the air immediately surrounding clouds, as we do not know sufficient about the conditions existing there. It may be as well to note here that on all occasions on which I have had an opportunity of making observations on this point, that the transition from moist air in the cloud to dry air immediately outside it was very rapid, so rapid as to give only a sharp outline.

Haze.

The conclusion to which these observations point is that haze is caused by the dust in our atmosphere, and that this dust has in almost all degrees of humidity more or less water attached to it. A thick haze may be the result of much dust and little moisture or of little dust and much moisture, but the moisture less than saturation. According to this view, in most conditions of our atmosphere, haze is but an attenuated or arrested form of condensation—arrested for want of moisture, or it may be a decayed form of condensation, that is, cloudy condensation changed to haze by the reduction of its humidity, possibly by rise of temperature, and the fog particles evaporated, till they hold only the water of attraction. If the humidity be decreased still further the haze clears more and more, but the dry dust still has a hazy or thickening effect on the atmosphere.

This conclusion is confirmed by an examination of the figures in the table, where it will be seen that whenever the air was dry and hazed there was much dust in it, and that as the dust decreased the haze likewise decreased. It is well known that hot weather is often accompanied by a thick haze. The explanation of this would appear to be, that during hot weather we have generally much dust in the air, and further as has been already explained

this dust owing to the high temperature has much moisture attached to it, even though the air is what is called dry. The conclusion we have come to is, that though there may be other causes of haze, yet dust is generally the cause of it.

Notes from the Rigi Kulm.

While at the Rigi Kulm, I observed a few meteorological phenomena which may be briefly referred to here, as they have some bearing on our subject. On my arrival at midday on the 21st of May, the hill top was covered with clouds, and we remained all day in a thick fog. However, as the afternoon advanced, an occasional glimpse was obtained of some of the higher Alps of the south, standing clear above the mass of cloud which filled in the valleys, and from time to time enveloped the place of observation. These unfortunately were but limited and passing glimpses, and all to the west was blocked with clouds. At last, however, shortly before the sun dipped below the horizon, the clouds began to clear overhead, and at last they rolled away, and the top of the Rigi stood clear above a billowy mass of clouds, and the western sun suddenly burst out brilliantly, dissipating in a moment the dull chill oppression of the fog. So sudden and welcome was the change, that the few who were waiting on the mountain top, in the almost forlorn hope of seeing the setting sun, raised a cheer of welcome; a cheer which rose simultaneously from every one, and could not have been more real, spontaneous, and stirring had we been a body of sun-worshippers welcoming our divinity.

The change, however, was momentary, for soon another mass of cloud passed across the hill top and obscured the view, but not for long, and it, in turn, was followed by another, each succeeding mass being less than the previous one, while each glimpse showed the west to be clearing, and at last no clouds rose above the mountain, and a magnificent view was obtained all round. Occasional openings in the clouds below gave passing views of the lakes and valleys low down near the base of the mountain, but on this occasion the great attraction was the cloud scenery. In most directions there extended a vast sea of clouds, the upper surface of which was slightly lower than the point of view. This wide area of clouds extended nearly level as far as the eye could see, its upper surface brilliantly

illuminated by the setting sun. It looked like a vast sea over whose waves gravitation had ceased to hold sway, and whose waters were luminous with condensed sunshine.

When the hill top began to emerge from the clouds and to be only occasionally covered with fog, as each fog mass cleared away to the east and let the sun shine on us, we saw our shadows cast by the setting sun on the retreating bank of cloud, "glories" surrounding the shadow-heads. Around the shadow of his head each observer saw a coloured halo; near the shadow-head there was a small luminous but colourless halo, and round this a rainbow-like circle of colours, violet being inmost and red outside. When this coloured circle was brilliant another circle of colours immediately outside was seen, and having the colours in the same order as the first.

After this display of colour in the east had come to an end, and no more clouds passed over the hill top, owing to the air being now clear to the west, an interesting display of colour took place in the west. At a point between the sun and the observer, there was a break in the cloud stratum, and the air seemed to be rising through this opening, and tearing the eastern edge of the cloud into fragments, which it carried from the dark shadow up into the sunlight. Here these fragments were rapidly dissolved in the dryer air above, and as they dissolved they gave rise in their dying moments to a brilliant display of opalescent colours, the different parts of the vanishing fragments taking on the most brilliant colours, and changing from one colour to another, and then vanishing so quickly that it was impossible mentally to follow the rapid display.

The display of opalescent colours in the sky has been commented on a good deal during the last few years, and Professor Tait has, I think, remarked that it must take place oftener than is imagined. There does seem to be a probability of it occurring often, but the conditions necessary for the eye perceiving it do not seem to be frequent. On this occasion the colours were seen against a dark background, formed by the shadowed eastern edge of the cloud. These colours were seen near the sun, at only a small angle from it, and it would appear they are produced in the same way as the colours in halos seen round the sun. In experimenting artificially with these halos, it may be observed that the particular colour does

not depend on the angle at which it is seen, but that it depends also on the size of the fog particle producing it. For instance, if we produce a cloudy condensation in a closed vessel placed between us and a light, the light is surrounded by a coloured halo, but the colour at any particular angle is not constant, but changes as we change the size of the fog particles; so that if we have two quantities of air, each with different sized particles in it, and both of these seen at the same angular distance from the sun, we will see different colours at the same angle and close to each other.

This mixture of colours producing the opalescent effect may be produced artificially. All that is necessary is to place a vessel of hot water nearly in a direct line between the observer and a bright light. As the condensed vapour rising from the hot water re-evaporates, the particles which were at first too large to give colour effects shine out in brilliant colours when vaporized to the size necessary to give the result. As the rising strata of air having different sized particles in them are close to each other, different colours are seen in close proximity, and as the size of the particles diminishes rapidly the colours change quickly. These colours are best seen when the steam is seen against a black surface, and the eye shaded from the direct light of the lamp.

On this evening on the Rigi brilliantly coloured rings were also seen surrounding the sun when just setting. The rings changed too rapidly for anything definite to be noted. These rings did not appear when a foggy cloud passed over the sun, but when there was only a fine haze; that is, the cloud particles were too large to produce colours, and it was only when they were nearly vaporized that the effects were produced.

All the brilliant display of colour we were favoured with on the 21st was gone by the following day. The morning of the 22nd was fine, the clouds had all settled down to a much lower level, to about 1000 feet below the top of the mountain, and the upper surface of the cloud stratum had now become more irregular, cumulus shapes tending to form at different places. During the rest of my visit to this station the clouds gradually cleared away and the weather was fine, and there were no other opportunities for seeing these interesting colour phenomena.

When working at an elevated position such as the Rigi Kulm,

one frequently observes a great difference in the condition of the upper and lower airs; and while making the observations on the transparency of the upper air which are entered in the table, a note was at the same time kept of the state of the lower air as it changed from hour to hour. As these latter observations bear directly on our subject, I shall here make some extracts from my note-book, and owing to the kindness of M. Billwiller of the Swiss Meteorological Office, Zurich, I am enabled to give the condition of the air with regard to humidity at different places at the lower levels on the dates corresponding to the Rigi observations.

Of the Swiss observations, those taken at Lucerne and Gersau are the most suitable for our purpose, as Lucerne is situated down the lake while Gersau is up the lake and on the opposite side of the Rigi. So that, while the Lucerne observations may be taken to represent the condition of the air towards the north of the Rigi, and the more open and inhabited parts of Switzerland, the Gersau ones will represent the condition of the air to the south and mountainous parts.

On the 21st, owing to the fog, no view could be got of the lower air. On the morning of the 22nd the view was still closed by the stratum of clouds covering the country to the north. In the afternoon, however, these clouds cleared away sufficiently to show the condition of the lower air. In my notes it is recorded that the air high up was fairly clear; but on looking down to lakes and valleys it was decidedly thickish, and in the evening the air to the north, east, and west was very thick, while to the south it did not thicken. Turning now to the Swiss Meteorological Report for this day, I find that at 1 P.M. the air to the north gave a depression of the wet bulb of 4°·2 C., while that to the south gave 3°·0 C. But as the evening advanced the humidity at Lucerne increased, and at 9 P.M. the depression was only 1° C., while at Gersau the depression had increased to 7°·6 C. Here we have a clearly indicated relation between the transparency and the humidity. The observations on the hill top being made in the afternoon, the air to the north would be more nearly saturated than it was when the 1 P.M. readings were taken; that is, the depression would be less than 4° C., and as stated the air looked thickish. In the evening it got very thick owing to the humidity increasing, the wet bulb depression decreasing to only

1° C. Now note the difference, the air to the south did not thicken in the evening, this was owing to the air on this side of the mountain getting dryer as the evening approached.

On the morning of the 23rd the air low down was thick all round, owing to the humidity being high on both sides of the mountain. At Lucerne the depression was only 2° C., and at Gersau only 1°·4. In the afternoon matters changed, and became much the same as they were on the afternoon of the previous day. The air to the south was clear low down, but to the north it was thick and hazy. To the north the wet bulb depression was 5°·2 C. at 1 P.M., and fell to 2° at 9 P.M., while in the south it was 3°·7 C. at 1 P.M., and increased to 7°·7 C. in the evening. Here again may be observed the same relation between the transparency and the humidity. The only difference of importance in the air on the 22nd and 23rd was a strange blackness in the air to the north on the 23rd which had not been observed on the previous day.

On the 24th and 25th nothing special was noted in the lower air in the different directions. On both of these days the air was clear both high up and low down, and on looking at the Swiss Meteorological Report I see the humidity was low on these days in both directions. At Lucerne the wet bulb depression at 1 P.M. was as much as 7°·8 C. on the 24th and 8°·4 C. on the 25th, and at Gersau it was 9°·1 C. and 9°·6 C. respectively, and in the evening the air kept fairly dry at both places. All through these observations of the air below the mountain we find the relation between the humidity and the transparency to hold good. Increased dryness was always accompanied by increased transparency. I was much struck while making these observations with the difference in the appearance of the air low down when looking north and when looking south of the mountain, and thought it might be due to the amount of dust in the two directions, the thickest air being towards the inhabited parts of the country. At the time I did not expect this difference in the humidity of the air at the two places which the Swiss observations show. It would be difficult to trace the cause of this difference in the humidity of the two places which are so near each other. Any attempt to trace it to direction of wind would be difficult owing to the influence of the surrounding mountains in producing local currents. It is quite possible the difference in the air to the north

and south of the mountain may have partly been due to the amount of dust in it, and the blackness noticed on the 23rd may have been due to this cause, as at Lucerne the wind in the early morning of that day was blowing from the north, and would bring polluted air up from the populated parts of the country. This north wind did not seem to penetrate into the mountainous parts south of the Rigi, as the wind at Gersau was south-west.

It may be asked, Is the air at the top of a mountain polluted to any extent by the valley air? Is the air on the mountain top the same as we breathe in the valley, only reduced in pressure? No very direct answer can be given to this question. There seems however, to be very little doubt that the valley air frequently rises to the tops of hills, but this valley air will generally be more or less mixed with the purer upper air, the amount of impure air depending on the height, the shape of the hill, and other causes.

It will be observed from the observations made on the 25th of May on the Rigi Kulm and at Vitznau at the foot of the mountain, that there was no very great difference in the amount of dust in the air at the two places. When allowance is made for the higher pressure at the lower station, there was less at the foot at midday than at the top in the morning. For practical purposes we may look on the dust on these two occasions as being the same, showing that in all probability the air tested at the foot of the mountain was the same as at the top. This, however, does not prove that the air of the valley ascended to the mountain top, because on this occasion a strong wind was blowing and the air was coming from the unpolluted area of the Alps, so that both the upper and lower air might have been free from local impurities. The observations made on Callievar showed there was much less dust in the air at the top than at the foot; still they also showed that the air came up from the valley as the numbers increased with a change of wind from a populated direction. The observations made on Finouillet show the air at that station to be frequently very impure from the ascent of valley air. The Rigi observations also show that in all probability the valley air came to the hill top of course more or less diluted with pure air. When there was little wind, as on the 21st, the amount of dust was small; but on the 23rd, when the wind began to blow, the amount of dust gradually increased to a

fair number of particles, and only decreased after blowing some time.

When on the Rigi it was also noticed when there were clouds in the valley, that the wind in driving them along, also drove them up the slopes of the mountains, sometimes to the very top. The condensation of the moist air in the clouds supplied some of the energy necessary for the ascent, as in some cases the clouds were seen to leave the hill-side and rise nearly vertically. We therefore seem to have very good reason for supposing that even at such an isolated position as the Rigi Kulm the air is more or less polluted with valley air. At stations on the tops, or on the slopes of long ridge-shaped hills, the air will be practically valley air when the wind is at right angles to the ridge.

There is another point which I noticed at this elevated situation, with which I shall close these Rigi notes. It has reference to the colouring of earth and sky seen at sunrise and sunset when observed from the top of mountains. I think the general impression is that the colouring is much finer on these occasions when seen from an elevation than from the valley. Now the result of my observations during my stay at the Rigi Kulm all point the other way.

The weather was favourable for seeing the sun rise and set on all the days I was there, and yet on none of them did I see any display of colour; indeed, I was much struck by the want of it. On the 24th, which was the finest sunrise during the time, the distant snows were tinged slightly red for a few minutes only while the sun was just on the horizon; immediately it got a very little clear of the horizon all colour was gone. The same was the case with the sunsets, greys predominated over other colours. Now during this time I was told that, as seen from Lucerne, the sunsets were remarkably fine for colour effects. This would seem to indicate that the colouring, at least under the conditions existing during my visit, was mostly done by the lower air—that the pure air between the mountains and the sun robbed the sun's rays of but little of their blue light, and that it was the lower impure air between the hills and the observer to which most of the colouring was due.

This supposition, that the lower dusty humid air is the chief cause of the colour in sunset effects is supported by other observations. When on the top of the mountain I frequently saw large

cumulus clouds; the near ones were always snowy white, while it was only the distant ones that were tarnished yellow, showing that the light came to these clouds unchanged, and it was only the air between the far distant clouds and the observer that tarnished them yellow, it required a great distance at this elevation to give even a slight colouring. It seems probable that the lurid light of thunder clouds owes its colouring to the dust and humidity of the lower air being excessive at the time, and that these clouds are really white, if we saw them near enough, it being the heavy moist air near the earth which changes the white to a reddish colour in its passage to the observer. We seem therefore to have good reason for supposing that the colouring at sunrise and sunset will be more brilliant when seen from the valley than from the mountain top.

Dust and Wind.

It may now be asked, Are there any other meteorological phenomena connected with the amount of dust in our atmosphere? It is evidently rather soon to begin drawing conclusions on the relation beween dust and many of the complex phenomena in nature, as the number of observations yet taken is far too small, and many of them are not free from local influences, which greatly reduce their value. There are, however, one or two points to which we may here refer; but it must be clearly understood that anything we have to say in this part of the paper cannot be taken as indicating anything final. They are given more as suggestions as to some of the many points in connection with our subject which await investigation.

One of the first things a meteorologist is likely to ask is, Do you find any relation between the amount of dust in our atmosphere and the distribution of pressure? Has the air in cyclonic and anticyclonic areas the same amount of dust? The answer to the question is easy and direct, but the interpretation of the answer is more difficult. There is no doubt from the observations here given that, as a rule there is less dust in cyclonic than in anticyclonic areas. For instance, when the observations were made at Kingairloch, on all days on which the number of particles was low, a cyclone was passing near, and on all days the number was great the circulation was anticyclonic or complicated.

To illustrate this, the diagram given on Plate No. 1 has been pre-

pared. At the bottom of the diagram are the dates on which the observations were made, and underneath the date is marked whether the station was in a cyclonic or anticyclonic area, A. indicating anticyclonic, C. cyclonic, and ? indicating that it could not be said to be in either the one or the other. In the diagram is shown the number of particles observed on each day. Each observation, given in the table referred to in a previous part of this paper, is indicated in the diagram by a small circle, the height of the circle above the base line indicating the amount of dust on the date, the scale being shown on the right hand side of the diagram. These dust observations are all connected by means of a dotted line. It will be noticed that in a general way the numbers were least in cyclonic areas.

Plate No. 2 illustrates the same thing for the Alford observations. Here the circulation was anticyclonic for the first four days, and the number of particles was high. A cyclone passed on the 9th, and the number fell to about one-third. The circulation was doubtful on the 10th and 11th, but on the following days it was generally anticyclonic, but it will be observed that the numbers were low during the beginning of this period. A curious point was the great increase which took place on the 16th. The number in the morning was 1125, but it rapidly increased during the day, and rose to 5700 in the afternoon. In connection with this it may be interesting to note that on the previous day the centre of the anticyclone had passed over this station, and on the 16th lay immediately to the east of it.

Taking now the Dumfries observations, which are shown on Plate No. 3, it will be noticed that the relation between the dust and the barometric distribution was more marked than in the previous cases. The circulation was cyclonic on the following dates—October 29th, 30th, 31st, November 1st, 2nd, 5th, 6th, and 15th, on all of which dates there was little dust. On only other two days was there little dust, namely, on the 25th October and 9th November. On these days the number was low and circulation anticyclonic. The high numbers were all got in anticyclonic or complicated areas.

Now comes the question, What interpretation are we to put on these facts? If the anticyclonic areas have more dust in them than

the cyclonic, then this at first sight might seem to indicate that anticyclonic air is more impure than cyclonic; that is, that air moving from areas of high pressure, which is generally supposed to come from the upper strata, has more dust than that near the earth, which would be equivalent to saying that meteors falling into our atmosphere produce more dust in the upper strata than is thrown into the lower by all the sources of pollution on the earth's surface in country districts.

There is, however, evidently another interpretation of these facts. One marked difference between a cyclonic and an anticyclonic area is, that the isobars in the former are much closer together than in the latter; that is, the barometric gradient is steeper in the cyclonic areas; or what this indicates is that there is more wind in the cyclonic than in the anticyclonic areas. That is to say, that in cyclonic areas there are high winds and little dust, while in anticyclonic areas there is little or no wind, and the amount of dust is great. Accepting this explanation of the greater or less amount of dust in the two areas, let us see how it is borne out by the observations.

In order to investigate this point we may lay off a series of points representing the distance between the isobars, on the different dates. The height of this curve would be inversely proportional to the velocity of the wind. Instead of using the barometric curves, we will use the observations of the velocity of the wind observed at the stations. These observations are entered in the diagram and connected by a fine line. It will be seen in diagram No. 1 that the fall in dust which took place on the 6th was accompanied by a rise of wind, and that the fall of the wind on the 9th was accompanied by an increase in the dust. The wind increased on the 10th, and the dust fell; but on the 12th, 13th, 15th, and 16th both curves rose and fell together. In diagram No. 2 it will be seen that both wind and dust rose and fell together from the 5th to the 9th, but on the 9th the wind increased and cleared away the high dust that had prevailed for some days. From the 9th to the 13th the dust increased whenever the wind got less, but on the 16th a great increase in dust took place with a slight increase in wind. When, however, we come to diagram No. 3, the relation between the wind and the dust is much more marked. It will be observed that almost every rise of wind is accompanied

by a fall in dust, and a fall in wind with an increase of dust. It seems therefore that the amount of dust is not so much a question of cyclonic or anticyclonic areas, as of wind velocity. It will be noticed that the exceptions in the Dumfries observations on the 25th October and 9th November, when the numbers were small and the distribution of pressure anticyclonic, have no existence where we adopt the wind explanation.

Having traced the connection between the number of particles and the velocity of the wind at low levels, the question still remains, What is the action of the wind? Two explanations seem possible. The wind will evidently mix the upper and lower airs, and may thus prevent the number being great, either by preventing the dust in the upper atmosphere from settling into the lower, or it may prevent the accumulation of local impurities, from which no locality is entirely free. The latter of these suggestions seems to be the more probable, as the atmospheric dust is so excessively fine that it is likely to take long to settle, and the duration of calms does not seem long enough for it settling from the upper strata. There is no doubt that for *all* the dust to settle out of even a few feet a very long time is necessary. It must, however, be kept in mind, that while all the dust takes long to settle, the heavier particles will fall quicker, and these no doubt in calm weather will add something to the impurity of the lower air. It, however, seems probable that the absence of wind will act chiefly in allowing local impurities to accumulate and keep near the ground. It may be as well to recall here, that on the Rigi Kulm, the lowest number of dust particles was recorded in calm weather; and that with increase of wind increase of dust took place, which increase continued till the wind had blown some time, thus indicating the power of the wind to make the upper air impure, and the complement of this will be to make the lower air purer.

That the increase of dust during calm periods is principally due to the accumulation of local impurities, seems to be supported by the difference, already pointed out, of the effect of wind as shown in the three diagrams. At Dumfries the relation between the amount of dust and wind velocity is constant all through, while at Kingairloch and Alford there were marked exceptions to it. Now, Dumfries is a much more polluted district than the others, and it is

polluted in all directions, whereas the other stations are not polluted equally in all directions. It seems, therefore, to be on this accumulated local impurity that the wind acts.

Another point which is shown in the diagrams Nos. 1, 2, and 3, is the influence of the direction of the wind on the amount of dust. At the foot of these diagrams are entered arrows showing the direction of the wind at the dates. The lower arrow shows the direction of wind as observed at the station. The upper one is from the weather charts issued by the Meteorological Office. These upper arrows will probably represent the general air circulation, while the lower ones will frequently be purely local. It will be observed that when the wind came from populous parts of the country, the numbers were generally large. Taking the Kingairloch observations,—all the country from the south to the east is thickly populated, but in all other directions there are but few habitations. On the 9th, with a light wind from the south, the number was great; whereas, with a light wind from the east or north, the number was less. The high numbers were all with southerly winds. At Alford, the most densely populated direction is from east to nearly south, and it will be observed that it was with winds from these directions that the greatest amount of dust was observed. The low numbers observed at this station, when the wind was slight from the 11th to the 14th, was probably due to the air coming from a comparatively poorly populated district. This seems to explain why on these days the number of particles was not great, as is generally the case when the wind falls. It will be noticed that the wind was south when the great increase of dust occurred on the 16th, with increase of wind. The effect of the direction of wind is not so apparent in the Dumfries observations, probably owing to this station being surrounded on all sides by towns and villages. It is, however, interesting to note that the northerly winds which were pure at the other stations were found to be impure here. The reason for this probably is, that many towns, as well as iron and other works, lie in that direction.

Fog.

The condition of the air during fog has been tested in a number of cases, of which no record is given here, and in all of them there was found a great quantity of dust. This is what we should now

expect, as fogs are formed when the air is still, and we have seen that when this happens the quantity of dust increases. The explanation of the formation of fogs would appear to be,—calms tend to increase the quantity of dust and moisture in the air near the ground, and as the dust increases the radiating power of the air it soon gets chilled to the condensing point, when fog forms. The density of the resulting fog depends in part on the quantity of dust present, as town fogs are thicker as well as blacker than country ones. These remarks only apply to radiation fogs, and not to fogs resulting from the mixing of hot and cold air, nor to fogs formed by reduction of pressure. Fogs seem to be much more frequent in town than in country air. This possibly may be due in part to the greater amount of dust in the city air increasing its radiating power, so causing it to become colder than country air, when there is no protection above it. If the air was clear and perfectly diathermanous, then the radiation would be only from bodies on the earth's surface. The air would then be cooled by these surfaces, and deposit its vapour on them as dew; but when there is much dust in the air, these particles acting as radiating surfaces get cooled, and the vapour being deposited on them, fog is the result.

Speculations.

Another question which may be asked is, Is there any relation between the amount of dust and the temperature of the air? It has been shown that there is a distinct connection between the amount of dust in the air and its transparency. We are, therefore, led to expect that the dust will have some effect on the temperature, by the alteration it will effect on the diathermancy of the air. In order to see if any information can be gained on this point from these observations, there are entered in the diagrams, Plates I, II and III, the maximum and minimum temperatures of the air on the days on which the dust observations were taken. It is evident that there are two distinct effects of the dust which we must distinguish. First, its effect on the heat received by the earth from the sun; and second, its effect on the heat radiated into space by the earth. We may expect the former of these effects will be best seen in summer, when the sun is high, while the latter will be best studied during night temperatures and during winter.

For investigating whether the amount of dust in the air has any influence on its temperature while the sun is shining, the only observations illustrating the point are those made at Kingairloch in July; the others made in September, October, and November, when the sun was low, shew no marked effect from its rays. When we examined the Kingairloch observations, we were struck by the marked relation shown between maximum temperature and the amount of dust. It will be observed that the curve of maximum temperature shows four maxima, and that the dust curve has also four maxima, and that three of these maxima of dust happen on the three days of maximum temperature. On the 4th, 9th, and 13th it will be seen both dust and temperature were above the average. The same thing though not so marked, may be noticed in the Alford observations. At the beginning of the observations both dust and temperature were high; both fell towards the end, and just at the finish both rose again.

These considerations point to a connection between the amount of dust and the temperature; the greater the amount of dust the higher was the maximum temperature. But what explanation are we to put on this? Is it a case of cause and effect? or are both due to one cause? or is it merely a coincidence? One would have expected that the phenomena are far too complex for any such effect to show out so clearly as it seems to do in these records. Amongst other influencing causes besides dust, there is the wind. The force of the wind does not seem to assist in explaining the agreement between the dust and the temperature curves in these cases; the direction of the wind, however, seems to have some effect. For instance, the wind was somewhat southerly with the high temperatures, but southerly winds at these stations are generally accompanied with much dust, while the northerly ones have a lower temperature, and little dust. This, however, does not dispose of the whole question, as the greater or less dust in the different winds may be in part the cause of the difference in their temperature. We do not find any influence of the dust on the maximum temperature in the Dumfries observations; when the sun is low, the direction of the wind seems to be the principal influencing cause. Most of the high temperatures occurred with southerly to westerly winds.

Humidity, no doubt, will have a powerful influence on the

temperature apart from its effect as a vapour, as a high humidity will increase the effect of the dust by increasing the size of the particles, while a low humidity will decrease the influence. The records of observation are as yet too fragmentary to warrant the drawing of any general conclusions on this subject.

Before the relation between the dust and the temperature can be settled with any degree of certainty, far more data are required. We would require to have, along with the dust tests, records of the strength of the sunshine taken by a more accurate method than the ordinary black-bulb vacuum thermometer; and also records of the hours of sunshine, the direction and velocity of the wind, not made at long intervals but continuously.

We shall now turn to the diagrams, and see if the earth's radiation at night is influenced by the amount of dust in the air, but our information is too meagre for a correct interpretation of the variations in temperature. The fall of temperature at night being due to the earth's radiation, before we can compare the cooling on different nights, continuous records of the amount of cloud in the sky are required, as well as the direction and velocity of the wind. As no records were kept of the clouds at night, and only imperfect ones of the wind, any conclusion we may draw from these curves will be of little value. The Dumfries observations will be most suitable for studying the night radiations, as they were taken during the long nights of winter. At first sight there does not seem to be any relation between the amount of dust and the cooling taking place at night. The reason for this is, that there are other influences at work besides dust, and one of these is the velocity of the wind—a high velocity preventing an accumulation of cold air near the earth's surface by mixing the cooled air with the warmer upper air. The Dumfries observations show that on all nights when there was wind the fall in temperature was slight, and that great falls only took place in calm weather. To get the effect of dust we must therefore compare only nights on which there was no wind or only slight airs. In order to assist our inquiry, there is placed in the diagram of the Dumfries observations another series of temperature observations taken by means of a minimum thermometer placed on the grass. These observations are shown connected by the curve underneath the curve of minimum air temperature. The difference between

these two curves, or the difference between the minimum air temperature and the minimum ground temperature, shows the intensity of the earth's radiation on the different nights. As the air is partly cooled by its passage over surfaces cooled by radiation, we might expect the fall of temperature from the previous day's maximum would be proportional to the difference between the minimum temperature and the minimum temperature on the grass. This, however, will only be the case when all other things are equal, such as amount and duration of cloud, diathermancy of upper air, radiating power of lower air, and velocity of wind.

Selecting the nights on which there was little wind, let us see if we can trace any effect due to dust. On the 30th October there was very little dust, and it will be noticed there was a great fall in the temperature on the following night, though the wind does not seem to have fallen very low. On this date the air was the purest observed at this station, and on the following night the radiation was the greatest observed. The fall in temperature of the air, though considerable, was not the greatest recorded; but we are not entitled to expect it would be, as the fall in temperature of the air depends on the duration as well as on the intensity of the radiation. No very satisfactory conclusion can be drawn from the other observations, as on the occasions when the amount of dust was low there seems to have generally been some wind, and this would check the fall of the thermometer on the grass. Further the dust observations were not taken at sufficiently short intervals for ascertaining whether the air at night had the same amount of dust as on the previous evening, or on the following morning. It may be as well to notice here, that on the 12th November there was a fair amount of dust, and yet the temperature fell considerably at night. The reason for this would seem to be, that though there was a good deal of dust, it was probably confined to the lower stratum of air, so that while the lower air was a good radiator from the amount of dust in it, it was unprotected above by dusty air. The reason for stating this is that on the following morning, tne 13th, when the amount of dust was great, there was a dense fog, but this fog was only low down, while immediately overhead the sky looked clear.

The relation between the dust and the night temperature appeared

in an interesting way in the Alford observations. For some days towards the end of the observations it will be noticed that the amount of dust was small, and the night minimum steadily decreased, while the small amount of dust continued. But on the 16th, the last day the tests were made, when the sudden increase of dust took place, there was a change also in the minimum temperature, which, instead of falling to a lower point, was the following night higher by nearly 12°. We must not, however, conclude that these changes of night temperature were entirely due to dust, as it will be noticed that the wind was occasionally northerly while the low night temperatures lasted, and that on the 15th the direction changed to southerly.

In the previous remarks there is something like a contradiction, which it may be as well to explain here. It is stated that the wind decreases the amount of dust, and also that it raises the minimum temperature. It is further stated that a decrease in dust is accompanied by a low minimum temperature. This apparent contradiction disappears when we consider that it is only when the wind has fallen that we can see the effect of the low dust in producing a low temperature.

We might sum up the results thus:—1st, During the summer there was generally a high maximum temperature when there was a maximum of dust in the air. 2nd, There was a tendency to a low temperature at night when there was little dust in the air. 3rd, During the time of the Dumfries observations the mean temperature was high, and the amount of dust great.

Stated in another way, it amounts to this, that dust increases the day temperature and checks the fall at night, thus increasing the mean temperature. If this be so, then it looks as if the dust absorbs the sun's heat during the day, but does not radiate the earth's heat at night. We might expect that the condition of air that got highly heated by the sun's rays would be greatly cooled at night by radiation. It will be as well, however, to remember that at present we are in great ignorance as to the diathermancy and radiating power of our atmosphere, and further it is easy to see that the heating and cooling will take place under different conditions. The sun's rays heat the air, and evaporate the moisture from the dust particles, and so tend to open the atmosphere to the

passage of heat through it; whereas, at night, an opposite action takes place. The chilling tends to deposit moisture on dust, and so impede terrestrial radiation.

In connection with this subject, there is a point that has been observed for some years, with regard to the earth's radiation, which, as is bears on our subject, we may refer to here. For some years I have taken the usual meteorological observations, and along with these there has been kept a record of the earth's radiation at night. When taking the readings in the evening, it has been noticed that on the evenings when there were no clouds, that the radiation effect was strongest just about sunset. If it was observed shortly afterwards, the radiation was almost always weaker, even though the sky kept clear. This would seem to indicate that the air is more diathermanous to terrestrial radiation just about sunset than it is later. The explanation which I would now offer of this is, that at sunset the air is more diathermanous than it is later, on account of the dust in the air holding little moisture. After the sun has been set for some time the air chills, and the dust particles increase in size by the vapour deposited on them; and this increase in size tends to check the free passage of the earth's heat into space. It is evident we cannot here attribute the decreased diathermancy, with the advance of the evening, to the action of the water in its gaseous form, as there is at that time no sudden increase in its amount in the air. It would rather appear to be due to change which then takes place in the condition of the moisture from vapour to liquid. I have said the air is more diathermanous just about sunset than it is later. Probably it is more diathermanous to the earth's variations during the day than it is at sunset; but observations on this point are difficult to make, owing to the complications introduced by the sun's heat radiated and reflected by our atmosphere.

At first sight, we might think that the truth or falsehood of these conclusions might be easily determined. If dust increased the day temperature and decreased the fall at night, then the mean temperature will be increased by a large amount of dust. Can it then be proved from general meteorology that this is the case? Do inhabited countries have a higher mean temperature than uninhabited ones, other things being equal; or has the temperature of these islands

risen since coal has come into such extensive use; or is the temperature of a place higher when the wind brings dust-laden air to it from a populous district than when it brings pure air; or has the temperature in the populous parts of New Zealand or Australia increased with the increase of population? Unfortunately this is a case where, while a positive reply might support the conclusion, a negative one does not weaken it, because the dust from human habitations keeps near the ground, and if there is no protection above it, its effect will probably be, not to keep up the night temperature, but rather to lower it, on account of the increased radiating power of the atmosphere caused by the dust.

There is still another reason why we must move here with caution. Should the possibility here shadowed forth become a reality, then we should find it entering the arena of one of the most highly contested fields of scientific speculation—we should find it claiming to be the great regulator of the earth's climate in bygone ages. Meteorologists would be disputing with astronomers, and claiming for their domain the cause of the great changes which geologists tell us have taken place in the earth's climate from time to time. They would no longer ask the astronomer if he could tilt the earth's axis so as to bring first one part and then another under the influence of the sun's rays. All the meteorologist would have to do would be in imagination to steer the solar system into a part of space where meteoric matter was absent. Were cosmic dust absent from the earth's atmosphere, the earth's heat would have a freer passage into space, and a glacial climate would be the result. To change this, it would only be necessary that the system passed to where meteoric matter was abundant, when the earth's atmosphere, now full of cosmic dust, would conserve the sun's rays and its own heat, and thus cause the glacial conditions to disappear.

Such are some of the problems suggested by this investigation, none of which are worked out, and some only suggested. It is of course possible, perhaps probable, that many of the conclusions here set forth may be quite wrong. The whole phenomena are too complex to be solved so easily, and it will require continued records of all the meteorological phenomena, extending over long periods, and under many different conditions, before many of these conclusions can be satisfactorily established.

Conclusions.

The following are some of the conclusions arrived at in this paper:—

1st, The earth's atmosphere is greatly polluted with dust produced by human agency.

2nd, This dust is carried to considerable elevations by the hot air rising over cities, by the hot and moist air rising from sun heated areas of the earth's surface, and by winds driving the dusty air up the slopes of hills.

3rd, The transparency of the air depends on the number of dust particles in it, and also on its humidity. The less the dust the more transparent is the air, and the dryer the air the more transparent it is. There is no evidence that humidity alone—that is, water in its gaseous condition, and apart from dust—has any effect on the transparency.

4th, The dust particles in the atmosphere have vapour condensed on them though the air itself may not be saturated.

5th, The amount of vapour condensed on the dust in unsaturated air depends on the "relative humidity," and also on the "absolute humidity" of the air. The higher the humidity and the higher the vapour tension, the greater is the amount of moisture held by the dust particles when the air is not saturated.

6th, Haze is generally produced by dust, and if the air be dry, the vapour has but little effect, and the density of the haze depends chiefly on the number of particles present.

7th, None of the tests made of the Mediterranean sea air show it to be very free from dust.

8th, The amount of dust in the atmosphere of pure country districts varies with the velocity and the direction of the wind. Fall of wind being accompanied by an increase in dust. Winds blowing from populous districts generally bring dusty air.

9th, The observations are still too few to afford satisfactory evidence of the relation between the amount of dust in the atmosphere and climate.

Table of the Number of Dust Particles in the Atmosphere.

Place.	Date.	Hour.	Number of Particles per c.c.	Wind.	Temperature.	Humidity.	State of the Air.	Remarks.
Hyères, . .	Mar. 19	10.0 a.m.	46,000	S.E. 1	...	11	Clear.	At window of hotel.
Finouillet, .	21	3.30 a.m.	5,000	W. 1	...	10	"	Near top of mountain.
Hyères, . .	22	11.0 a.m.	9,000	W. 1	...	11	...	At window of the hotel.
"	"	"	5,600	"	...	"	...	" "
Finouillet, .	23	3.15 p.m.	4,100	S.W. 1	...	9	Thickish.	Near the top of the mountain.
"	"	3.45 p.m.	19,000	...	...	...	Very thick.	" beginning to rain.
"	25	4.0 p.m.	3,550	W. 1	...	13	Clear.	At top of mountain.
"	"	"	10,000	...	...	...	"	Number very variable.
La Plage, .	29	12.0	2,150	S.E. 1	...	13	"	Taken on the beach.
"	"	1.30 p.m.	4,800	S.E. 0·5	...	"	"	Wind fallen.
"	"	1·45 p.m.	1,850	"	...	"	"	At 100 yards from shore.
"	"	2.0 p.m.	1,800	"	...	"	"	At 250 " "
Finouillet, .	Apr. 3	3.30 p.m.	4,400	N.W. 6	61·2	13	Extly. clear.	Observed on top of mountain.
"	"	"	4,500	"	"	"	"	" "
"	4	"	25,600	W. 3	55	8·5	Clear.	" "
"	"	"	24,400	"	"	"	"	Wind direct from Toulon.
Cannes, . .	10	11.0 a.m.	1,550	"	65·5	13	Very clear.	Observed on the top of La Croix des Gardes.
"	"	"	1,600	"	"	"	"	
"	11	11.30 a.m.	1,950	S.W. 3	54	2	Very thick.	" "
"	12	10.30 a.m.	140,000	E. 1	63	10	Very clear.	" "
"	"	"	150,000	"	"	"	"	Large number due to wind blowing from the town.
"	"	"	125,000	"	"	"	"	
"	13	11.30 a.m.	10,600	S. 0·5	56	7	Thickish.	On pier in west bay.
"	"	"	12,000	"	"	"	"	On shore in west bay.
Mentone, . .	25	3.0 p.m.	1,200	N.W. 1	62	12	Very clear.	Observed on hill about 1000 feet high to the N.W. of Mentone.
"	"	"	7,200	S.E. 1	"	"	"	
"	27	10.0 a.m.	5,000	S.E. 0·5	64	13	"	Observed in small boat outside the pier.
Bellagio, . .	May 3	11.15 a.m.	10,000	S.W. 0·5	64	7	Thickish.	Observed at open window.
"	"	3.0 p.m.	10,000	S. 0·5	63	6	"	Observed on the top of the Villa Serbelloni.
"	"	3.30 p.m.	9,500	"	"	"	"	
"	6	3.0 p.m.	3,000	S.W. 0·2	60	3	Thick.	At open window, rain.
"	7	2.30 p.m.	5,600	S. 0·2	66·5	7·5	Thick.	Taken at window.
"	8	12.30 p.m.	2,900	S.W. 1	62	5	Very thick.	" " been rain.
Baveno, . .	13	10.30 a.m.	9,000	N.W. 0·2	70	10	Thick.	Taken at window.
"	"	3.30 p.m.	6,780	...	...	...	"	" "
"	14	11.0 a.m.	3,200	N.W. 3	56·5	3·5	Very thick.	" " wet night, raining, distant thunder.
"	"	3.0 p.m.	3,800	N.W. 4	57	4	"	At window, still raining.
"	15	10.30 a.m.	10,000	Calm	58	2	Thickish.	" " raining, calm.
"	"	12.15 p.m.	6,120	...	...	...	...	" " "
"	"	2.0 p.m.	4,500	E. 0·1	59	2	Extly. thick.	" " "
"	"	3.0 p.m.	2,900	E. 0·2	...	...	Very thick.	" " "
"	16	10.0 a.m.	5,600	N.W. 3	58	3·5	Very thick.	" " wet morning.
Simplon Pass,	"	3.30 p.m.	850	N.W. 0·5	...	...	Clear.	Taken a short way up the Simplon Pass.
"	"	3.45 p.m.	546	...	...	...	"	" "
"	"	4.0 p.m.	14,000	S.E. 0·5	...	...	"	Increase due to change of wind.
Baveno, . .	17	10.0 a.m.	6,250	S.E. 0·5	65	6	Medium.	Taken at window.
"	"	11.15 a.m.	6,600	...	"	...	"	" "
Locarno, . .	18	4.0 p.m.	1,150	N. 2	...	6	Thickish.	Taken on hill side at 600 feet above the lake.
Rigi Kulm, .	21	2.0 p.m.	1,300	Variable	46	1·5	Thick fog.	Taken on the top of the hill.
"	"	3.30 p.m.	285	...	"	...	"	" "
"	"	4.30 p.m.	210	...	45	1·5	"	" "
"	"	"	227	...	"	...	"	" "
"	22	9.30 a.m.	850	S.E. 2	51	6	Clear.	" "
"	"	"	700	..	...	...	...	" "
"	"	11.30 a.m.	434	...	...	...	"	" "
"	"	"	518	...	...	...	...	" "
"	"	3.30 p.m.	1,450	...	55	4	Haze.	Occasional passing fog.
"	"	"	1,550	...	...	...	"	Thunder storm to the east.
"	"	"	1,550	...	...	...	"	
"	"	"	2,050	...	...	...	"	" "
"	"	"	2,200	...	...	...	"	" "
"	"	"	2,350	...	...	...	"	" "
"	"	4.30 p.m.	2,050	...	50	4	"	" "

NUMBER OF DUST PARTICLES—*continued.*

Places.	Date.	Hour.	Number of Particles per c.c.	Wind.	Temperature.	Humidity.	State of the Air.	Remarks.
Rigi Kulm, .	May 23	10.0 a.m.	850	S.E. 3	54·5	8	Clear.	Some clouds below the level of the hill top, and on the mountains to south-east.
"	"	"	1,100	...	...	...	...	
"	"	"	580	...	...	...	...	
"	"	"	515	...	...	...	...	
"	"	"	850	...	...	...	...	
"	"	"	571	...	...	...	...	
"	"	11.0 a.m.	1,080	...	...	...	...	
"	"	"	1,036	...	...	...	...	
"	"	3.30 p.m.	850	S.E. 2	...	...	Very clear.	Thunderstorm to the south-east, but distant.
"	"	"	850	...	...	...	...	
"	"	"	850	...	...	...	...	Low clouds all gone.
"	"	"	600	...	...	...	...	
"	"	"	600	...	...	...	...	
"	"	"	840	...	...	...	...	
"	"	"	665	...	...	...	...	
"	"	4.30 p.m.	850	...	53·5	9·5	...	
"	24	10.0 a.m.	685	S.E. 1	53·5	9·5	Very clear.	
"	"	"	665	...	...	...	...	
"	"	"	665	...	...	...	...	
"	"	11.0 a.m.	617	...	58	11	...	
"	"	4.0 p.m.	395	...	...	...	...	
"	"	"	392	...	...	...	...	
"	"	"	350	...	...	...	...	
"	"	5.0 p.m.	575	...	...	...	...	
"	"	"	592	...	51	8	...	
"	25	9.0 a.m.	532	S.S.E. 1	53·5	10	Very clear.	Taken on the top of the mountain.
"	"	"	540	...	...	...	...	
"	"	"	560	...	...	...	...	
"	"	"	580	...	...	...	...	
"	"	10.0 a.m.	547	...	...	...	...	
Vitznau, . .	"	12.30 p.m.	585	S.E. 3	...	14	Very clear.	Taken at the level of the lake.
"	"	"	616	...	...	...	...	
Lucerne, . .	"	5.0 p.m.	616	S.E. 3	..	14	Very clear.	Taken on lake side at a distance of one mile to the windward of the town.
"	"	"	650	...	...	...	...	
"	"	"	652	...	...	...	...	
"	"	6.30 p.m.	1,970	...	...	...	...	At hotel window over garden.
"	26	10.30 a.m.	23,000	N. 0·5	...	13	Medium.	" "
"	"	2.0 p.m.	7,500	N.E. 0·5	...	...	...	" "
"	"	3.0 p.m.	2,330	...	...	...	...	Taken in a field about a mile to the windward of the town.
"	"	"	2,400	...	...	...	...	
"	"	"	2,160	...	...	...	...	" "
"	"	"	2,750	...	...	...	...	" "
"	"	"	3,000	...	...	...	...	" "
"	"	4.0 p.m.	3,660	...	...	...	...	
Eiffel Tower,	29	10.0 a.m.	41,000	S. 3	...	...	...	Taken at an elevation of 200 m.
"	"	10.15 a.m.	52,000	...	...	...	...	Taken at the top of the tower immediately underneath the lantern for the electric light at an elevation of nearly 300 m.
"	"	"	15,000	...	...	...	...	
"	"	"	3,300	...	...	...	...	
"	"	"	6,600	...	...	...	...	
"	"	"	104,000	...	...	...	...	" "
"	"	"	45,400	...	...	...	...	" "
"	"	"	62,000	...	...	...	...	" "
"	"	"	22,400	...	...	...	...	" "
"	"	12.0 a.m.	2,850	S. 5	...	...	...	" "
"	"	"	226	...	...	...	...	Heavy shower of rain.
"	"	"	24,000	...	...	...	...	Rain ceased.
"	"	1.30 p.m.	42,000	S. 6	...	...	...	Taken at an elevation of 200 m.
"	"	2.15 p.m.	56,000	...	...	...	...	Taken at an elevation of 115 m.
"	"	"	43,000	...	...	...	...	" "
Paris, . . .	"	4.0 p.m.	210,000	S. 4	...	...	...	Taken in the garden of the Meteorological Office.
"	"	"	134,000	...	...	...	...	
"	"	"	160,000	...	...	...	...	
"	Mar. 15	11.30 a.m.	92,000	N.E. 2	...	...	...	" "
London, . .	June 3	10.0 a.m.	102,000	...	...	...	...	Taken at window in Victoria Street.
"	"	"	140,000	...	...	...	...	
"	"	12.0 a.m.	88,000	W. 2.	...	...	...	Taken in a garden, the wind

Number of Dust Particles—*continued.*

Places.	Date.	Hour.	Number of Particles per c.c.	Wind.	Temperature.	Humidity.	State of the Air.	Remarks.
London, . .	June 3	12.0 a.m.	84,000	...	...	...	...	blowing direct from Battersea Park.
,,	,,	,,	116,000	...	...	...	...	
,,	,,	,,	48,000	W. 3.	...	...	...	Wind increased.
,,	,,	1.0 p.m.	84,000	...	...	...	...	
,,	,,	2.30 p.m.	150,000	...	...	...	...	At window in Victoria Street.
Kingairloch.	July 3	12.30 p.m.	3,150	S.E. 1.	73	13	Haze.	Observations made at about 50 feet above the level of the sea.
,,	,,	,,	3,000	...	...	...	...	
,,	,,	7.0 p.m.	2,150	...	...	...	...	
,,	,,	,,	2,250	...	...	...	...	
,,	,,	,,	2,200	E. 1.	...	...	...	
,,	4	11.0 a.m.	3,600	...	70	10	Thick haze.	
,,	,,	,,	4,000	...	...	...	...	
,,	,,	7.0 p.m.	1,550	W. 1.	...	...	...	
,,	,,	,,	1,250	...	...	...	...	
,,	5	10.0 a.m.	1,250	S.E. 1.	64	6	Thick.	
,,	,,	,,	1,550	...	...	...	...	
,,	,,	,,	1,500	...	...	...	...	
,,	6	10.30 a.m.	1,000	Variable.	61	4	Thick.	
,,	,,	,,	1,000	...	...	...	...	
,,	,,	1.0 p.m.	400	N.W. 2.	...	...	Clear.	Clouds on hill tops.
,,	,,	,,	325	...	...	...	...	
,,	,,	,,	336	...	...	...	...	
,,	,,	,,	385	...	...	...	...	
,,	,,	1.30 p.m.	303	,,	60	6	Clear.	
,,	,,	4.30 p.m.	205	...	...	...	...	
,,	,,	,,	250	...	...	...	...	
,,	8	10.0 a.m.	1,650	N.W. 2.	61	9	Medium.	Cloudy, with occasional showers.
,,	,,	,,	1,450	,,	...	...	...	
,,	9	10.0 a.m.	2,500	S. 0·2	69	8	Medium.	Dull morning.
,,	,,	...	2,500	...	...	...	...	
,,	,,	...	2,400	...	...	...	...	
,,	,,	12.30 p.m.	1,230	S. 0·5	64	9	Clear.	
,,	,,	6.0 p.m.	2,650	E. 1	61	6	Thickish.	
,,	10	11.30 a.m.	814	E. 3	59	4	Medium.	Been raining.
,,	12	5.30 p.m.	710	S.E. 0·5	60	5	Clear.	Fine, but cloudy.
,,	13	10.30 a.m.	3,000	S. 1	62·5	5	Thick.	Fine morning.
,,	,,	,,	3,000	,,	...	...	,,	
,,	,,	1.0 p.m.	2,900	,,	65	5·5	,,	
,,	,,	,,	3,100	,,	,,	,,	,,	
,,	15	11.30 a.m.	425	Variable.	56	4	Clear.	Thunder in distance.
,,	,,	,,	550	,,	,,	,,	...	Carry S.W.
,,	,,	,,	550	,,	,,	,,	...	
,,	,,	2.0 p.m.	1,900	N.W. 1	57	4·5	Thick.	Air became much thicker.
,,	,,	,,	1,850	,,	...	...	...	
,,	16	10.0 a.m.	1,400	Variable.	59	6	Medium.	Fine, but clouded.
,,	,,	,,	1,300	,,	,,	,,	...	
,,	,,	1.30 p.m.	800	,,	62	7	Very clear.	Carry from north.
,,	,,	,,	850	,,	,,	,,	...	One of the clearest days at this place.
,,	17	10.0 a.m.	510	E. 0·2	59	8	Very clear.	Cloudy, carry from north.
Ben Nevis, .	Aug. 1	1.0 p.m.	335	S.W. 1	45	2	Hazy.	Fairly clear, Schiehallion and Skye hills visible.
,,	,,	3.0 p.m.	473	,,	45·5	2·6	,,	
Alford, . .	Sept. 5	11.0 a.m.	2,000	Variable.	71·2	10·7	Thick.	Dull, foggy morning.
,,	,,	5.30 p.m.	1,800	...	...	...	,,	
,,	6	10.30 a.m.	3,850	E. 1	71·2	10·9	,,	
,,	7	10.0 a.m.	2,750	E. 0·5	62	6·2	Very thick.	
,,	9	9.0 a.m.	3,000	S.W. 1	68	10·1	Thickish.	Cloudy.
Callievar, . .	,,	12.0 a.m.	262	S.W. 3	,,	,,	Clear.	Clouds clearing away.
,,	,,	,,	336	,,	,,	,,	,,	
,,	,,	2.0 p.m.	475	S.W.W. 3	,,	,,	,,	Clouds nearly gone.
,,	,,	,,	475	,,	,,	,,	,,	
Alford, . .	9	4.30 a.m.	900	S.W.W. 2	...	...	...	
,,	10	9.30 a.m.	1,750	W. 0·5	67	6·5	Thickish.	Dull day.
,,	,,	6.0 p.m.	530	S.W. 1	...	...	Hazy.	,,
,,	11	10.0 a.m.	1,125	Variable	67	5·9	Thick.	Dull morning.

NUMBER OF DUST PARTICLES—*continued.*

Places.	Date.	Hour.	Number of Particles per c.c.	Wind.	Temperature.	Humidity.	State of the Air.	Remarks.
Alford, . .	Sept. 12	10.0 a.m.	650	E. 0·5	59·7	4·5	Thickish.	Dull, cloudy morning.
"	13	11.0 a.m.	1,400	E. 0·2	55	1·6	Thick.	" "
"	"	2.0 p.m.	900	Calm	...	...	"	Light rain all day.
"	14	10.0 a.m.	850	E. 0·2	52·1	4·9	Clear.	
"	"	5.30 p.m.	850	N.W. 0·2	...	...	"	
"	16	10.0 a.m.	1,125	S. 1	63	8·6	Medium.	Fine bright morning.
"	"	5.30 p.m.	5,700	S.W. 0·5	...	...	Extly. thick.	Gradually became very thick.
Dumfries, .	Oct. 24	10.30 a.m.	600	E. 1	51	7·5	Clear.	Sunshine and cloud.
"	25	10.0 a.m.	1,025	N.E. 0·5	...	...	Clear.	Fine day.
"	26	"	1,475	N.E. 1	44	6·5	Medium.	Fine day, with clouds in morn-
"	"	2.45 p.m.	850	E. 2	47·5	7·5	"	ing and sunshine in after-
"	"	3.30 p.m.	1,200	"	47	7	"	noon.
"	28	10.0 a.m.	5,500	N.W. 1	45	6·5	Thickish.	Cloudy all day.
"	29	"	11,000	S.W. 0·2	45·2	1·2	Very thick.	Dull most of the day.
"	"	3.30 p.m.	1,200	S. 0·8	50·5	4·6	Medium.	
"	30	10.0 a.m.	325	S.W. 5	52	1	Very thick.	Raining.
"	"	3.30 p.m.	235	"	52·5	1	Thick.	Been wet and stormy.
"	31	10.0 a.m.	1,600	S.W. 0·5	47·8	2·6	Clear.	Fine day.
"	Nov. 1	10.0 a.m.	550	S.W. 6	42	0·8	Very thick.	Rainy, stormy.
"	"	3.30 p.m.	395	W. 4	44·2	1·8	Thickish.	Rain ceasing.
"	2	10.0 a.m.	635	N.W. 3	47	5	Clear.	Very fine day.
"	4	"	4,600	N.W. 0·2	43·5	1·5	Thickish.	Wind variable, fine day.
"	5	"	1,350	S.W. 0·2	42	0·2	Very thick.	" "
"	6	"	1,635	S.W. 0·2	47·5	2·5	Medium.	" "
"	"	2.30 p.m.	1,360	W. 1	50	4·6	Clear.	" "
"	7	10.0 a.m.	1,250	W. 1	56	1·5	Thickish.	Fog on hills. Dull all day, with
"	"	3.0 p.m.	1,400	W. 1	56·5	1·5	"	slight rain.
"	8	10.0 a.m.	5,350	S.W. 0·2	49·8	1·8	Thick.	Wind variable, dull day.
"	"	2.30 p.m.	2,670	"	50	0·5	Thick.	" "
"	9	10.0 a.m.	625	W. 2	54	2·0	Very clear.	Fine day.
"	"	3.0 p.m.	750	W. 3	53·8	2·5	Very clear.	"
"	11	10.0 a.m.	2,250	W. 0·5	50·2	1·4	Thickish.	Dull day.
"	"	2.45 p.m.	1,375	"	49·2	1·2	Very thick.	
"	12	9.30 a.m.	2,500	S. 0·2	49·5	0·5	Extly. thick.	Dull day.
"	13	10.30 a.m.	8,600	Calm	39	0·0	Extly. thick.	Very thick fog.
"	"	3.0 p.m.	8,000	Calm	39·3	0·0	"	Thick fog, but clear overhead.
"	14	10.0 a.m.	9,900	"	45·1	1·6	Extly. thick.	Not so thick as previous day.
"	"	2.30 p.m.	7,070	"	47·8	2·0	"	
"	15	10.0 a.m.	975	S.W. 0·5	51·5	0·5	Thick.	Dull most of day.
"	"	3.0 p.m.	900	"	52·7	1·8	...	Air got slightly clearer.
"	"	4.0 p.m.	650	S.W. 1	52·9	2·9	Thickish.	Air now much clearer.
"	16	10.0 a.m.	7,700	N.W. 0·2	43·5	2·5	Thick.	Cloudless all day.
"	"	12	4,700	"	46	3·5	Thickish.	Fine day.
"	18	10.0 a.m.	9,000	Calm	46	0·9	Very thick.	Dull all day.
"	19	10.0 a.m.	11,500	S.W. 0·2	48·8	1·0	Extly. thick.	Dull.

APPENDIX.

Being desirous of getting more observations on the effect of humidity when the temperature is low, in order to check the conclusion come to in the paper, I made a visit to the west of Scotland in the end of January last, in the hope of finding the conditions suitable for the purpose. Garelochead was selected for these observations. This situation is fairly free from local pollution so long as the wind is not south, south-east, or east. In all other directions it is fairly free from contamination.

The observations were generally made on the hills above Garelochead; the site for each day being always selected to the windward of the houses. Unfortunately, the weather was not suitable for the special purpose intended, as during most of the time the temperature was high and the weather extremely stormy. As, however, the observations were taken in exceptional weather, and the results show peculiarities, they may be thought of sufficient interest to be recorded here.

The observations were begun on the 23rd of the month. On this day the number of particles was 2360 per c.c., and the temperature 34°; but unfortunately the wet-bulb depression is doubtful, owing to the temperature of the wet-bulb being below the freezing point. The wet-bulb fell to 32°, where it remained steady. As the temperature was falling fast, the conditions would be all changed before the water on the wet-bulb was all frozen, and it was therefore useless to wait to see how far it would fall below 32°. Fortunately, I had a hygroscope exposed at the same time, and it showed a reading corresponding to a wet-bulb depression of fully 3°.

This is the only observation obtained at this situation suitable for illustrating the influence of temperature on the effect of the humidity, and it confirms the conclusion previously arrived at. We have shown that when there was a wet-bulb depression of about 4°, and a little over 1000 particles per c.c., that the air was thick if the temperature was 60° or more; clear or medium if it was about 50°; and, in the case here recorded, the air was clear, with nearly double the number of particles, and the air not quite so dry, but at a temperature of 34° It seems probable from this that observations made on Ben Nevis and in cold climates generally

will not show the influence of humidity so markedly as those made in warm climates.

In making out the table given with this paper, the humidity might have been shown in other ways. For instance, in place of entering the wet-bulb depression, the relative humidity or the dew-point of the air might have been given. It was, however, thought better to enter the wet-bulb observations themselves, since either of the others can be easily calculated from them. At ordinary temperatures the relative humidity and the dew-point are approximately proportional to the depression of the wet-bulb, the dew-point being nearly twice as much as the wet-bulb below the temperature of the air. But for low temperatures the proportions do not agree so well. For instance, the dew-point at the temperature of 60° is 1·88 times the wet-bulb depression below the temperature of the air; at 50° it is 2·06 times; while if the temperature is 34°, it is 2·77 times. So that for low temperatures part of the clearness is due not only to the lower vapour pressure, but also because, for a given wet-bulb depression, the dew-point is really farther below the temperature of the air with a low than with a high temperature. For instance, taking the above examples with a wet-bulb depression of 4°, the dew-point at 60° is 7°·5 below the temperature of the air, at 50° it is 8°·2, while at 34° it is fully 11°; and when the temperatures are still lower the difference increases rapidly. The result is that, when there is a small difference between the wet and dry bulbs while the temperature is very low, the dew-point is a considerable distance below the temperature of the air. For instance, at a temperature of 20° and a wet-bulb depression of only 1°, the dew-point is fully 8° below the temperature of the air. If the temperature had been 53°, in order that the dew-point might be as many degrees below the temperature of the air, the wet-bulb depression would require to be 4°.

We must therefore only use the wet-bulb depression for comparison when the temperature is over 50°, below that temperature allowance must be made. In the future, perhaps, either the depression of the dew-point or the relative humidity would be a better figure to use in these tables.

Returning now to the table in the appendix, it will be observed that on the 24th that there were 725 particles per c.c. and a depres-

sion of only 2°·5, and the air was clear. The tests on this day also confirm our conclusion regarding the less effect of the humidity when the temperature is low, but the numbers are not extreme enough to show it in a marked degree.

The observations made on the 25th are interesting, as they were taken in exceptional conditions, and gave exceptional results. This day will long be remembered as one of the stormiest of a very stormy January. From the table it will be seen that at no time in the day was the number of particles over 1000 per c.c., and it will be also noticed that the air was dry, giving a wet-bulb depression of fully 5°; and yet, contrary to all previous experience, the air was thick, the hills at little over three miles being invisible. As to the cause of this thickness with few particles and dry air, it is at present difficult to say anything definite. It, however, seems possible that the size and the nature of the particles may have had something to do with it. The gale would enable the air to hold in suspension *large* particles of dust, and the atmosphere would also contain salt spray. It was noticed that the air had a peculiar glistening appearance, quite unlike the usual look of thick air. On the evening of this day the air became much clearer, due possibly to there being fewer salt particles, as the wind had veered, and the air had to travel a greater distance over land before reaching the place of observation. Part of the greater clearness was doubtless due to the increased dryness which took place in the afternoon.

On the 27th the wind was again high, blowing strong from the north-west. The number of particles had fallen to 250 per c.c., and the air had become much clearer.

The 28th is principally remarkable for being the day on which was recorded the smallest number of particles yet observed. The storms had now passed, and the wind fallen to a gentle air from due north. These tests were made on the hill-side to the north of Whistlefield. On this occasion there was great difficulty in getting clear of artificial pollution, the great purity of the air enabling the existence of a house at a distance of half a mile to be easily detected. The site selected for the observations had to be changed a number of times before one was found free from local impurities, from the instrument revealing the existence of dwellings hidden away at a distance among the hills.

The air on this day being very pure, all the tests made are entered in the table, and not averaged, as is frequently the case on the other days. Of course, each of the tests given is as usual an average of ten readings. The lowest test gave 86 per c.c., being the number for the purest air yet observed by me.

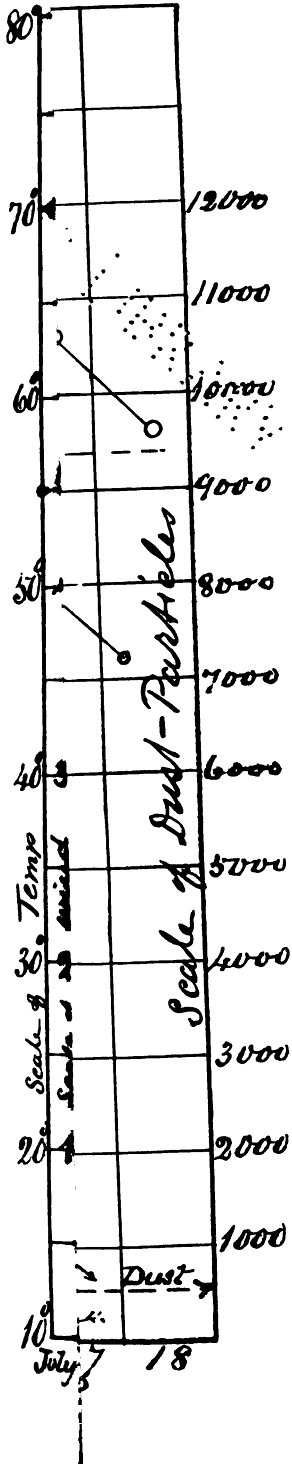
When this small number was obtained the barometric distribution was very complicated. While the tests were being made the cyclone of the previous days had passed away to the north-east, and an anti-cyclone had appeared on the west coast of Scotland. The air tested seems to have been true anti-cyclonic air, which was but little contaminated by local pollution.

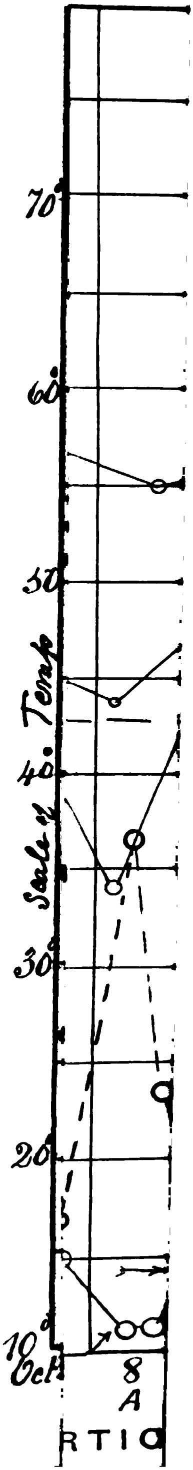
On the following day the wind was still northerly and slight, and the dust had begun to accumulate, the numbers increased to nearly three times what they were on the previous day. The air remained clear, as it was both pure, dry, and cold.

TABLE SHOWING THE NUMBER OF DUST PARTICLES IN THE ATMOSPHERE.

Place.	Date.	Hour.	Number of Particles per c.c.	Wind.	Temperature. °	Humidity. °	State of the Air.	Remarks.
Garelochead,	Jan. 23	3.30 p.m.	2,360	N.W. 0·2	34	3·5	Clear.	
,,	24	11.45 a.m.	725	S.W. 1	40	2·5	,,	Sky clouded.
,,	25	11.30 a.m.	700	S.W. 6	50	5	Thick.	Hills visible to only 3 miles.
,,	,,	1.0 p.m.	925	W.S.W. 7	49·8	5·8	,,	,, ,,
,,	,,	3.30 p.m.	925	W.S.W. 6	48	6·5	Medium.	,, to more than 10 miles.
,,	27	11.30 a.m.	250	N.W. 6	41	3	Clear.	Passing showers.
,,	28	,,	116	N. 1	40	4·5	Extly. clear.	Clearest day observed.
,,	,,	...	100	...	...	,,	,,	Sky cloudless and sun brilliant.
,,	,,	...	93	...	...	...	,,	
,,	,,	...	95	...	...	...	,,	
,,	,,	...	88	...	...	...	,,	
,,	,,	1.0 p.m.	86	...	39	4·75	,,	
,,	29	11.30 a.m.	253	N.W. 1	42	3·75	Very clear.	Sky dull, upper air thickening.

PLATE I.


80°
70°
60°
50°
40°
30°
20°
10°
12000
11000
10000
9000
8000
7000
6000
5000
4000
3000
2000
1000
Scale of Dust-Particles
Scale of Temp
Scale of wind
Dust
July 7
18


70°
60°
50
Scale of Temp
40°
30°
20
10°
Oct
8
A
RTIO

Larix europæa as a Breeding-Place for Hylesinus piniperda. By William Somerville, D.Œc., B.Sc.

(Read July 7, 1890.)

The special point that I wish to bring before the Society relates to a case of the common larch being made use of by *Hylesinus piniperda* for purposes of oviposition. As occasion demands, this insect has been found to utilise as a breeding-place every species of Pinus, but, so far in Europe or North America, no case has been noted of any trees belonging to the genus Larix having been similarly attacked. In Asia an observer, namely, Middendorff, has recorded one case which came under his notice in the district of the Boganida, in Siberia, in 71° north latitude (*Sibirische Reisen*, Band iv. Theil i. p. 603).

In the beginning of April of this year, in the Upper Ward of Lanarkshire, on a south-west slope, at an elevation of some 800 feet, I found that several larches, which had been felled during last winter, were attacked by large numbers of this insect. In its company I also found *Hylastes palliatus*, but by far the greater number of galleries were the work of *Hylesinus piniperda*. During the past three months these trees have been kept under close observation, with the result that I find one or two particulars in which the attack of this insect on the larch differs from its mode of attacking the Scots pine.

The greater abundance of fluid resinous matter in the larch, as compared with the Scots pine, seems to have considerably interfered with the work of forming galleries. I noticed that all the trees lying in the wood were not attacked, but only those at one side, where they were within the shade cast by a dense wood of pines situated to the south. This, I believe, to be due to the fact that the cambial activity and formation of resinous solutions were retarded in these trees owing to their not being directly reached by the sun's rays; whereas the cambium and cortex of those trees fully exposed to the sun were so saturated with resin as to be safe from attack. Even in some of the trees attacked I found unfinished galleries quite full of resinous secretions, and containing the dead bodies of the male and female insects, which had doubtless been drowned or suffocated by the resinous exudations.

Another peculiarity in the galleries made by the parent insect in the larch, is that in a large number which I have examined I have only on one occasion found an air-hole. This is somewhat remarkable, because, when the galleries are made in the natural food-plant of the insect, at least one air-hole is present in each, and, as a general rule, they contain two or three. I am somewhat at a loss to account for this, because, half-smothered by resin as the insects are in their galleries, the admission of air would appear to be a most desirable consideration. It is probable that here also the great amount of resin in the cortex of the larch interferes with the normal formation of the galleries.

As to the cause of *H. piniperda* attacking the larch, I believe a satisfactory reason can be given. About ten years ago the southern counties of Scotland were visited by a succession of exceptionally severe gales, which overturned enormous numbers of pines and other trees. Partly owing to the glutted state of the market, and partly to the difficulty experienced in dealing with such a large amount of fallen timber, the woods were allowed to remain undisturbed in their devastated condition for a number of years. These dead and dying trees furnished an exceptionally favourable breeding-place for *H. piniperda*, which consequently increased at a prodigious rate, each average-sized pine being capable of producing, it is said, as many as 80,000 insects. Within the past year or two the last of this fallen timber has been removed, with the result that the huge army of forest insects, by which the country is overrun, cannot be accommodated with the breeding-places which they prefer, and have therefore been compelled to oviposit on what they must consider most unsuitable material. Thus, owing to stress of circumstances, *H. piniperda* has been driven to attack the larch, and in this country I have also found Scots pines, not exceeding eight years of age, infested by it, although hitherto trees of a less age than fifteen years have seldom been known to be attacked.

Researches on Micro-Organisms, &c. Part III. By Dr A. B. Griffiths, F.R.S. (Edin.), F.C.S. (Lond. & Paris), Member of the Physico-Chemical Society of St Petersburg, &c.

(Read March 16, 1889.)

Dr P. W. Latham, in the Harveian oration at the Royal College of Physicians, on October 17, 1888, supported my own researches (which have already been published in the *Proc. Roy. Soc. Edin.*, vol. xiv. pp. 97–106, and vol. xv. pp. 33–63), on the subject of "disease germs." He says:—"We might hope to find that the action and growth of the bacilli [of phthisis] might be inhibited by certain substances, and then, by *injecting these substances into the blood*, disease might be prevented, or if disease existed it might be arrested or cured."

These remarks are almost in the same language as those I have already had the honour of presenting to the Royal Society of Edinburgh.

In Part II. of this paper (*Proc. Roy. Soc. Edin.*, vol. xv. p. 34), the following words will be found:—"The principle of these researches is to find some germicidal agent capable of destroying the microbes of disease, which have been proved to reside in the blood, and are the causes (directly or indirectly) of certain contagious diseases. At the same time, an aqueous solution of such an agent, while destroying the microbes of disease, must have very little or no detrimental action upon the blood. Having found such a substance, the rationale is to inject (hypodermically) a solution of the microbe-destroyer directly into the blood. By so doing, the destruction of the pathogenic organisms *in situ* would be the result."

Having once more alluded to the rationale of the proposed method of treating those contagious diseases whose microbes reside in the blood, we now describe a further series of experiments performed on living micro-organisms.

I. The Alkaloids of Living Microbes.

The origin of the "vital" alkaloids, or those produced by living microbes, is not thoroughly understood. They may be the products

of the metabolism of protoplasm, or the result of the decomposition of albuminoid substances.

Since my last paper (*loc. cit.*) was published, I have discovered a new alkaloid produced by *Bacterium allii** growing on nutrient agar-agar, having the following symbolic formula, $C_{10}H_{17}N$. The results of the analyses of this substance were as follows:—

	I.	II.	III.
C =	79·47	79·50	79·48
H =	11·26	...	11·24
N =	9·27	...	...

The alkaloid (extracted by Gautier's process) is a white solid—soluble in water, alcohol, ether, and chloroform. It crystallises from water in microscopic needles belonging to the prismatic system. These crystals are extremely deliquescent. The alkaloid forms a platino-chloride having the composition $(C_{10}H_{17}N, HCl)_2 PtCl_4$. It is a yellow-coloured compound, slightly soluble in cold water, but soluble in hot water.

II. Action of Certain Antiseptics and Disinfectants upon Microbes.

I have already shown (*loc. cit.*) the action of disinfectants on a number of organisms, but a much more efficient germicide has recently been reported on by Bouchard.

Bouchard (*Comptes Rendus*, vol. 105, p. 702) found that 0·33 gram. of β-naphthol in 1000 c.c. of various cultivating media prevented the development of several species of "bacteria," including those of—

Bacillus anthracis.
Bacterium pneumonicum agile.
Bacillus typhosus (weak cultivations).
Bacillus tuberculosis (retards the development).
Bacterium choleræ gallinarum.

This substance may be introduced into the stomach of a rabbit to the extent of 3·8 grammes per kilogramme without producing death. "The fatal dose for a man of 65 kilogrammes would therefore be

* Several tubes containing growths of *B. allii* were exhibited at the meeting of the Lincoln Scientific and Literary Society on March 3, 1888.

more than 250 grammes, *and it is only slightly more poisonous when injected subcutaneously.*"

These results have been confirmed by Dr J. Maximovitch (*Comptes Rendus*, vol. 106, p. 1441). We may, accordingly, conclude from the above investigations that β-naphthol is a true germicide, and is capable (to a certain extent) of being injected into the system *without* toxic effects.

We have thus at our command several reagents which have been proved to destroy the microbes of some of the worst forms of disease which afflict man and animals. We may then, perhaps, look forward to physicians putting into practice the ideas given in these papers of destroying the disease germ by the injection of a germicidal agent.

At this point, we consider the action of certain reagents on the growth and development of various microbes.

(a) *Bacillus tuberculosis.*

I have found (for a second time) that solutions of salicylic acid (natural), potassium iodate, and sodium fluosilicate (in the proportions already mentioned in this paper) destroy *Bacillus tuberculosis.*

It is impossible to inoculate tubes containing sterilised blood serum *plus* 3 per cent. of salicylic acid with *Bacillus tuberculosis* from a pure cultivation or from sputum (from the worst form of phthisis). This clearly shows the germicidal nature of the reagent. I have performed the same experiments with *Bacterium allii*, *Bacillus subtilis*, *Bacillus œdematis maligni*, and Deneke's *Spirillum*, and found that their growth was inhibited in a precisely similar way.

(b) *Bacteria of Infantile Fæces.*

On April 13, 1888, Dr Baginski read a paper, before the Berlin Physiological Society, on the cultivation of *Bacterium lactis* (obtained from infantile fæces) in a sterilised solution of pure milk-sugar.

After fermentation, *no* lactic acid but *acetic acid* was found as the result of the life-work of the micro-organism in this particular medium. The microbe in this medium is both aerobic and anærobic, and always produces acetic acid. The gaseous products of the fermentation are carbonic acid gas, hydrogen and marsh gas.

Baginski says that when the microbe is cultivated in gelatine,

acetic acid proves a germicide. Therefore the product of its own life-history plays the part of a powerful toxic agent on the further life of the bacterium.

From Baginski's researches may we not draw the inference that in certain contagious diseases "the turning-point for the better" is due to products formed by the microbes, which act detrimentally upon any further growth and development.

In a paper read before L'Académie des Sciences on October 26, 1886, Pasteur stated that "a large number of microbes apparently give rise, in the media where they grow, to substances which have the property of opposing their own growth." And again, "might it be that rabies virus was made up of two distinct substances, the one *living* and capable of multiplying in the nervous system, the other *not living*, but capable still, when in suitable proportions, of arresting the development of the first"?

(c) *The Microbe* (?) *of Hydrophobia.*

M. Galtier of Lyons found that salicylic acid injected hypodermically was quite inefficient in preventing the development of hydrophobia.

In a communication to the International Medical Congress (Copenhagen meeting), held on August 11, 1884, M. Pasteur stated that "the process for isolating the microbe is still imperfect, and the difficulties of its cultivation outside the bodies of animals have not yet been got rid of, even by the use, as pabulum, of fresh nervous matter." Then, again, in a paper read before L'Académie des Sciences on February 25, 1884, Pasteur stated that in microscopical sections of a rabid medulla there are numerous minute granules, "suggesting the idea of a microbe of extreme tenuity, in shape neither a bacillus nor a diplococcus; they are like simple dots."

From the researches of Pasteur, we note that the microbe of rabies has a very different life-history from other micro-organisms. The principal seat of the virus is in the central nervous system; even after the injection of the poison into the blood system, "the spinal marrow is the region first attacked, the virus locating itself and multiplying there before spreading to other parts." Therefore, the virus of rabies *does not reside in the blood*,—which accounts for Galtier's failure with salicylic acid.

From these details, it appears that the medium in which the undiscovered microbe of rabies resides is essentially nervous matter, and not in the blood system. Hence the hypodermic injection method is useless as a means for preventing or curing this terrible disease.

(d) *Various Micro-Organisms.*

The following micro-organisms—

(1) *Bacillus floccus* (obtained from garden soil),
(2) *Bacillus toruliformis* (obtained from garden soil),
(3) *Bacillus tardecrescens* (from ammonium carbonate solution),
(4) *Micrococcus candicans* (from the atmosphere),
(5) *Micrococcus rosaceus* (from the atmosphere),
(6) *Micrococcus chlorinus*,—

as well as those mentioned in my last paper (*loc. cit.*) on this subject, have all been destroyed by the germicides already mentioned.

III. Vitality of Certain Micro-organisms.

In continuation of the experiments recorded in Part II. of this paper, I have found that the vitality of certain micro-organisms is considerable.

Portions of a pure cultivation of *Micrococcus chlorinus* (growing on white of egg) were mixed with calcium sulphate and calcium carbonate (the mineral ingredients were previously sterilised at a temperature of 135° C.) and placed in a number of sterilised tubes, which were then hermetically sealed. Twelve of these tubes each contained from 8 to 10 grammes of the mixture. Twelve sterilised tubes, not hermetically sealed (see Part II. of this paper), also contained the same quantity of the mixture. The twenty-four tubes were kept at a dry heat of 32° C. from one to eight months. Two hermetically sealed tubes and two of the "open" tubes were opened after an exposure at 32° C. for one month. Four tubes containing sterilised nutrient beef-broth were inoculated from the contents of the tubes. In the two inoculated from the "open" tubes, growths of *Micrococcus chlorinus* (proved by staining, microscopical and macroscopical appearances) made their appearance after *nine* days' incubation. Growths of *Micrococcus chlorinus* also made their appearance in the

two tubes (inoculated from the contents of the sealed tubes) after thirteen days' incubation. Four more tubes were opened after an exposure for two months at 32° C. (dry heat). Inoculations from two "open" tubes revealed the vitality of this microbe after sixteen days' incubation;—and inoculations from the two sealed tubes proved the vitality of the micrococci after the lapse of twenty-one days' incubation. The remaining tubes were examined in a similar manner after the lapse of three, four, six, and eight months respectively.

After an exposure at 32° C. (dry heat) for three, four, and six months, the vitality of *Micrococcus chlorinus* was *not* destroyed. And finally, after being dried up for eight months, the vitality of this microbe was completely destroyed; for no growths made their appearance in sterilised nutrient beef-broth kept at a temperature between 32° and 36° C. for nearly three months.

A similar series of experiments were performed with other micro-organisms (the final inoculations being made in different media so as to suit each case). The results were as follows:—

	After an Exposure at 32° C. (dry heat) for:—							
	1 month.	2 months.	3 months.	4 months.	5 months.	6 months.	7 months.	8 months.
Micrococcus rosaceus,	L*	L	L	L	...	D	D	D
Bacterium allii,	L	L	L	L	L	L	D	D
Micrococcus prodigiosus,	L	L	...	L	...	D	D	D
Bacillus tuberculosis,	L	L	L	L	D*	D	...	...

From these experiments, it will be seen that various microbes are capable of being dried up in the dust of the atmosphere for several months without losing their vitality.

M. Duclaux (*Comptes Rendus,* vol. 100, pp. 119 and 186) proved that the germs of certain species of Tyrothrix, especially *Tyrothrix scaber,* are not destroyed by at least three years' exposure in a dry state to air of a tropical temperature, but were killed by exposure to direct sunlight at the same temperature for some weeks.

* L = living. D = dead.

IV. Action of Freezing Mixtures upon certain Micro-organisms.

During these researches, I found that certain micro-organisms were killed at low temperatures.

Tube cultivations of the following microbes were used in these experiments :—

(1) *Bacillus tuberculosis.*
(2) *Bacterium allii.*
(3) *Bacillus subtilis.*
(4) *Spirillum tyrogenum.*

The temperatures employed were obtained by using the following freezing mixtures :—

Mixture.	In the Proportion of (by weight).	Minimum Temperatures recorded.
Ice,	2	−18° C.
Salt,	1	
Water,	1	−15° C.
Ammonium nitrate,	1	
Sodium sulphate,	8	−17° C.
Hydrochloric acid,	5	

After the micro-organisms had been exposed to the above temperatures for *several* days, a number of tubes containing sterilised agar-agar, blood serum, &c., were inoculated from the contents of the above tubes (*i.e.*, the tubes exposed to the low temperatures).

From these experiments the following results were obtained :—

	At −18° C.			At −17° C.			At −15° C.		
	For 1 day.	For 3 days.	For 14 days.	For 1 day.	For 3 days.	For 14 days.	For 1 day.	For 3 days.	For 14 days.
Bacillus tuberculosis,	L*	D	D	L	D	D	L	D	D
Bacterium allii,	D*	D	D	D	D	D	L	L	D
Bacillus subtilis,	L	D	D	L	L	D	L	L	D
Spirillum tyrogenum,	L	D	D	L	L	D	L	L	D

* L = living. D = dead.

It appears that certain microbes are able to withstand the severity of a low temperature, although their vitalities (proved by a longer period of incubation) are impaired.

It has been stated that *Bacillus anthracis* retains its vitality after being exposed to a temperature of − 110° C.

V. Electrical Experiments on Certain Micro-organisms.

The following results concerning the action of the electric current on the vitality of certain micro-organisms form a continuation of those recorded in Part II. of this paper. The experiments were performed upon pure cultivations of three micro-organisms.

(1) *Bacillus tuberculosis* growing in previously sterilised fluid blood-serum was killed by an E.M.F. of 2·16 volts.

(2) *Bacterium allii* growing in previously sterilised pork-broth (neutral) was killed by an E.M.F. of 3·3 volts.

(3) *Bacillus subtilis* growing in previously sterilised pork-broth (neutral) was killed by an E.M.F. of 2·72 volts.

The temperature of the laboratory was 17° C., and the current was allowed to pass for ten minutes in each case.

The "electrified" micro-organisms were then "transplanted" to a certain number of tubes containing the above cultivating media. After an incubation, at 35° C., for twenty days, *no* growths made their appearance in any of the tubes.

The above experiments show that the electric current proves a powerful germicide.

VI. The Micro-organisms of the Atmosphere.

The method used in these experiments for determining the number of "colonies" (of micro-organisms) in a known volume of air, was that devised by Hesse (*Mittheilungen aus dem Kaiserlichen Gesundheitsamte*, 1883, bd. ii.).

After August 6, 1888, a similar method to the one described by Dr P. F. Frankland in *Nature* (vol. xxxviii. p. 235), was used in the experiments.

The following three tables speak for themselves:—

(*a*) The Air of Lincoln.

(*Average number of "colonies" in three gallons.*)

Place.	Year 1887.											
	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
(1) Top of hill (*near the Cathedral*)	3	6	14	16	19	25	34	No experiments made in Lincoln.	30	28	12	4
(2) Base of hill, (*Broadgate*)	18	26	30	41	50	62	65		59	57	19	17

(*b*) The Air of Paris.

(*Average number of "colonies" in three gallons*).

Place.	Situation in Paris, &c.	August 1887.
(1) Cimetière du Père la Chaise (*near tomb of Abelard and Héloïse*),	E.	96
(2) Boulevard St Germain,	Central	104
(3) Forest of Ville d'Avray,	S.W.	81
(4) Rue de Rennes,	Central	99
(5) Palais du Trocadéro,	W.	50
(6) Park of Versailles (*near Palace*),	S.W.	78
(7) St Cloud (*near Palace ruins*),	S.W.	82
(8) Boulevard Voltaire,	E.	100
(9) Cimetière Montparnasse,	S.	98
(10) Cimetière Montmartre (*near tomb of Offenbach*),	N.	95
(11) Parc des Buttes Chaumont,	N.E.	80

(*c*) The Air of London.

(*Average number of "colonies" in three gallons.*)

Place.	July 1888.	August 1888.
(1) Forest Gate (*Essex*),	64	79
(2) City (*near Bank*),	85	110
(3) West End (*Piccadilly*),	80	96
(4) East End (*near Mint*),	88	160

The conclusions to be drawn from this portion of the research are the following :—

(1) That there are a larger number of micro-organisms in the summer than either in the spring or winter. They appear to reach their maximum number during the month of August.

(2) The number of micro-organisms found in the atmosphere *decreases* the higher one ascends. Hence near the Lincoln Cathedral there are fewer micro-organisms in the atmosphere (on any given day) than in the valley of the Witham (*Table* (*a*)). The same remark also applies to the number of micro-organisms found in the atmosphere on the top of the Trocadéro Palace, Paris, where there are fewer than in a low-lying, but crowded, thoroughfare like the Boulevard St Germain.

(3) There are a larger number of micro-organisms in the atmosphere of crowded centres than in less densely populated centres; hence more in cities than in the country.

(4) The number of micro-organisms are fewer when the air is "at rest" than at any other time. For instance, there were fewer micro-organisms in the atmosphere of tranquil places, like the cemeteries of Père la Chaise, Montparnasse, and Montmartre, than in busy streets like the Rue de Rennes, Boulevard Voltaire, &c.

During my researches, attempts have been made to isolate pathogenic microbes from the atmosphere, but up to the present without success.

VII. Hypodermic Injections of Salicylic Acid for Phthisis.

R. Wood, M.D., L.R.C.P. (mentioned in Part II. of this paper) reports that he has cured a girl of phthisis by injecting a saturated solution of salicylic acid into her system. He says, in a letter dated September 2, 1888:—"*I have been injecting salicylic acid twice a week on a girl for one year, and she is now better.*"

Dr Wood is injecting salicylic acid into the blood of another phthisical patient. On September 4, 1888, he sent me a bottle labelled—"*Sputum of a girl, Webb, phthisical night sweats, age* 14. *Has spit blood. I am injecting a saturated solution of salicylic acid daily.—R. Wood.*"

I found in the sputum a considerable number of tubercle-bacilli.

There is little doubt that salicylic acid (natural) is a powerful germicidal agent.

In Part II. of this paper I gave the details of certain injection

experiments performed on Mr John Snodgrass. His case was that of a "lung disease" of thirteen years' standing, which became distinctly tubercular several years ago.

Although Mr Snodgrass was greatly relieved, and his life prolonged, by the injection methods (*Proc. Roy. Soc. Edin.*, vol. xv. p. 53), yet there was hardly any hope of a permanent cure in his case. He suffered from a complication of diseases, which caused him to discontinue the injections of salicylic acid (Part II., p. 62). The disease proved fatal on May 24, 1888.

The administration of salicylic acid in the form of pills will have no effect in curing phthisis. During its passage through the system salicylic acid becomes altered.

It combines with glycocine forming salicyluric acid, thus—

$$C_6H_4\left\langle\begin{matrix}OH\\COOH\end{matrix}\right. + CH_2\left\langle\begin{matrix}NH_2\\COOH\end{matrix}\right. = C_6H_4\left\langle\begin{matrix}OH\\CO.NH-CH_2-COOH\end{matrix}\right. + H_2O.$$

The salicyluric acid so formed passes off by the urine.

The rationale of my method is to destroy the "germs" of disease in the blood (*i.e.*, where they reside).

Since the reading of my last paper on this subject, before the Royal Society of Edinburgh, I have injected into my own system two other substances (besides salicylic acid), without any ill effects. One of these was 20 minims of a 0·4 per cent. solution of sodium fluosilicate. We have already seen that this substance is a powerful germicidal agent, capable of destroying *Bacillus tuberculosis.*

The other substance injected was 20 minims of an 1·0 per cent. solution of sodium salicylate.

Sodium salicylate is far more soluble in water than salicylic acid, but it is not a germicidal agent.

Sodium compounds (as a general rule), with non-poisonous acid radicles, have little or no action on the blood when used in small quantities.

It was the suggestion of Dr Wood that sodium salicylate might prove a valuable antiseptic, but my experiments have all failed to show that it possesses this property.

On the other hand, Dr Grandeau and others have shown that when *potassium* compounds are injected into the blood, they paralyse the heart and striated muscles (Kemmerich's *Archiv fur Physio-*

logie, bd. ii. p. 49). This action occurs even when dilute solutions are injected into the system. The blood contains normally a small percentage of potash, but certain limits must not be exceeded, or poisonous effects are the results.

From the above it will be seen that a solution of potassium iodate * (which is a germicidal agent of great power) is useless for injection purposes.

VIII. Sugar and Cellulose, two Products formed during the Life-History of Bacillus tuberculosis.

About six years ago M. Pouchet extracted sugar ($C_6H_{12}O_6$) from the lungs of phthisical patients. And Dr Freund discovered that *Bacillus tuberculosis* forms cellulose in the organs and blood of tuberculous persons. I have also proved the presence of cellulose in the *sputa* of phthisical patients (*Proc. Roy. Soc. Edin.*, vol. xv. p. 36). My experiments on this point have been repeated several times, and always with the same results.

IX. The Infectious Nature of Human Sputum.

There is little doubt that the expectorations of phthisical patients are of an infectious nature; as it has been proved that when susceptible animals are fed on tuberculous matter, they become infected with the disease.

I think it accordingly advisable for physicians attending phthisical patients to give full instructions for nurses and others to disinfect the urine, fæces, and sputa, so that there may be no chance of infection. It would not be a difficult task for nurses attending consumptives to immerse all handkerchiefs after use in water containing carbolic acid.

X. Bacillus tuberculosis in the Breath of Phthisical Patients.

I have already shown that the *saliva* of consumptive patients often contains the germs of phthisis (*Proc. Roy. Soc. Edin.*, vol. xv. p. 55). I have also obtained growths in blood-serum, exhibiting all the macroscopic and microscopic appearances of tubercle bacillus from the breath exhaled by a person in a state of advanced phthisis.

* Already mentioned in this paper.

XI. Soluble Ferments produced by Microbes.

Professor Giglioli (*Fermenti e Microbi*) describes the production of *soluble* ferments by living micro-organisms. What have the soluble or non-organised ferments (produced by pathogenic microbes) to do with contagious diseases? Before the formation of alkaloids, is a soluble ferment excreted by each microbe? Is it possible that the ferment causes the chemical disintegration of albuminoid molecules with the ultimate formation of alkaloids?

We know that vegetable diastase (produced in the first instance from albumin by the action of living cells) is capable of converting starch into dextrose, and cane-sugar into dextrose and levulose:—

$$(a)\ C_6H_{10}O_5 + H_2O = C_6H_{12}O_6.$$
$$(b)\ C_{12}H_{22}O_{11} + H_2O = C_6H_{12}O_6 + C_6H_{12}O_6.$$

These substances are of a definite chemical composition and formed by the action of non-organised ferments. Therefore, may we not infer that the alkaloids of disease are possibly produced by the soluble ferments secreted by pathogenic microbes.

The researches of Giglioli (*Fermenti e Microbi*) and Schiavuzzi (*Atti della R. Accademia dei Lincei*, 1886) have thrown considerable light on this all-important but most obscure subject.

Recently, N. Kravkoff (*Journal of the Russian Physico-Chemical Society*, vol. xx. [part 8], pp. 623—632) has made a thorough inquiry into the properties and action of *pure* vegetable diastase.

The production of *alkaloids* by pathogenic microbes is most important; for "one of the most interesting facts observed in the growth of septic micro-organisms is this, that the products of the decomposition started and maintained by them have a most detrimental influence on themselves, inhibiting their power of multiplication; in fact, after a certain amount of these products has accumulated, the organisms become arrested in their growth, and finally may be altogether killed. Thus the substances belonging to the aromatic series,—indol, skatol, phenol, and others which are produced in the course of putrefaction of proteids,—have a most detrimental influence on the life of many micro-organisms, as has been shown by Wernich and others" (*Klein*).

Quoting from Part II. of this paper:—"If the soluble zymases

produced by living pathogenic microbes are the real cause of disease, the hypodermic injection method steps in, for many substances are known to intérfere with the action of soluble ferments. The destruction of the microbes prevents the formation of soluble zymases or alkaloids, and any given contagious disease (under these circumstances) would be at an end. Nature would then have a chance of restoring to their 'normal standard the lowered vitality which enabled the microbes to get a footing.'"

Conclusion.

There is little doubt that the most rational method of treating contagious diseases (whose microbes reside in the blood) is by the injection of some germicidal agent. When the microbes are destroyed the disease would be at an end.

My investigations on this subject are still incomplete, but I sincerely hope that they will be found interesting and full of suggestions for the pathologist and physician.

My apology for bringing them (in their present incomplete condition) before the Society, is in the words of Lavoisier:—"A man would never give anything to the public if he waited till he had reached the goal of his undertaking, which is ever appearing close at hand, and yet ever slipping farther and farther as he draws nearer."

On the Solution of the Three-Term Numerical Equation of the *n*th Degree. By the Hon. Lord M'Laren.

(Read March 17, 1890.)

The method of solution developed in this paper, while perfectly general in its character as regards three-term equations, may also be considered as a solution of the complete equation of the 5th degree; because, by known methods resulting from the theory of equations, every equation of the 5th degree is reducible to three terms.

I may be permitted here to make an introductory observation on the supposed superiority of an algebraic to a numerical solution. The algebraic solution has undoubtedly an aspect of theoretical

completeness; but in point of utility there is very little difference. Supposing that an equation of the 5th degree could be solved algebraically so that x is expressed as the 5th root of a function of known quantities, a, b, and c. If we write for a, b, and c their values, and proceed to find the 5th root of the function, there is no known method of obtaining the root except by the use of logarithms. Now the extraction of the 5th root by the division of a logarithm is equivalent to the solution of the transcendental equation $a = 10^{\frac{1}{5}y}$, which is only possible because tables of values of y to the argument x have been computed.

The solution which I here offer is obtained by the use of differential logarithms, and is not different in character from that which I have considered.

It is not an approximate solution, as I understand the expression; because the value found for x is obtained by one operation, and is incapable of being made more accurate by applying corrections to it.

The value found for x is, however, only true on the assumption that the tabular values are true values. But this limitation of the accuracy of the solution is not a consequence of any imperfection of the method, but results solely from the fact that the tabular quantities which are assumed to be known are quantities which do not admit of exact numerical expression, or which, in other words, cannot be completely expressed in a series of powers of the number *ten*.

Method of Logarithmic Differences.—These functions, which are sometimes called Gaussian logarithms, were really invented, or first computed, by Lionelli (*Supplément Logarithmique*, Bordeaux, 1802–3; German translation, 1806). Gauss's table was published in 1812, with the knowledge, on Gauss's part, of what Lionelli had done. It may be convenient here to point out how these functions are constructed, and how the tables are used:—

Let $u = \log(a+b)$; then $u - \log a = \delta u$. The quantity δu is the logarithmic difference, or tabular quantity, which has to be added to $\log a$, to make up u, or $\log(a+b)$. In this paper the logarithmic differences are denoted by the symbol, $\lambda o\gamma_b^a$; *i.e.*, $\log\left(1+\frac{b}{a}\right)$; the Greek character being used to distinguish these from common logarithms.

If instead of $\log(a+b)$ we take such a quantity as $\log(\alpha x^n + \beta x^p)$, the logarithmic difference may be denoted by the symbol, $\lambda o\gamma_p^n$, where

$$\lambda o\gamma_p^n = \log(\alpha x^n + \beta x^p) - \log \alpha x^n = \log\left(1 + \frac{\beta}{\alpha} x^{p-n}\right) \quad . \quad (1).$$

In the tables of *Logarithms of Addition*, the logarithmic difference is found from the argument $(\log a - \log b)$, or in the second case from $(\log \overline{\alpha x^n} - \log \overline{\beta x^p})$, which I denote by the symbol $\mathfrak{m}_p^n$ or $\mathfrak{m}_p^n$, where

$$\mathfrak{m}_p^n = \log\left(\frac{\alpha}{\beta} . x^{n-p}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

It seems desirable that quantities which are of general application should be denoted by symbols not liable to be mistaken for the symbols of other things, which is my reason for proposing this notation.

Similarly, for logarithmic differences of subtraction, we have

$$\lambda o\gamma_p^n = \log(\alpha x^n - \beta x^p) - \log \alpha x^n = \log\left(1 - \frac{\beta}{\alpha} . x^{p-n}\right) . \quad . \quad (3).$$

$$\mathfrak{m}_p^n, \text{ as before} = \qquad \log\left(\frac{\alpha}{\beta} . x^{n-p}\right) . \quad . \quad . \quad . \quad (2).$$

In the numerical examples given in this paper I have made use of Zech's *Tables of Logarithms of Addition and Subtraction* (Berlin, 1863), in which the argument ($\mathfrak{m}$) is given to five significant figures with proportional parts, and the logarithmic difference ($\lambda o\gamma$) is given to seven significant figures.

The property of these functions which attracted my attention, and led to this new application of them is this: that for any function, $\log(a+b)$, or $\log(\alpha x^n + \beta x^p)$, the logarithmic difference does not depend on the absolute values of a and b, or αx^n and βx^p, but solely on their ratio.

For if we denote a logarithmic difference (as above) by $\lambda o\gamma_{\mathrm{B}}^{\mathrm{A}}$ we have, $\lambda o\gamma^{\mathrm{A}} = \log(\mathrm{A}+\mathrm{B}) - \log \mathrm{A} = \log\left(\frac{\mathrm{A}+\mathrm{B}}{\mathrm{A}}\right) = \log\left(1 + \frac{\mathrm{B}}{\mathrm{A}}\right)$, whatever may be the absolute values of B and A.

By means of this property we are enabled to eliminate the powers of x from the logarithmic equation, and to obtain equations between

$\lambda o\gamma$, $\mathfrak{m}$, and known coefficients, by which $\lambda o\gamma$ and $\mathfrak{m}$ are first found, and afterwards x.

Solution of the Equation of the 5th degree.

It is known from the theory of equations that by a not difficult operation any quintic equation may be reduced to three terms. I suppose this preliminary reduction performed, and the equation given in the form

$$ax^5 \pm \beta x^4 = 1.$$

If we take the upper sign there are two possible solutions derived from the relations, $\log(ax^5 + \beta x^4) = \left\{ \begin{array}{l} = \log ax^5 + \lambda o\gamma_4^5 \\ = \log \beta x^4 + \lambda o\gamma_5^4 \end{array} \right\} \begin{array}{l} (a). \\ (b). \end{array}$

If the equation has more than one real root, which is not generally the case where the two powers of x are both positive, one of these may be found from each form.* If there be only one root, the first formula will be used if the quantity ax^5 be $< \beta x^4$, and *vice versa*, as is usually evident from the values of the coefficients.

(1) From the given equation we have $\log(ax^5 + \beta x^4) = 0$, and from formula (a) we have, as above,

$$\log(ax^5 + \beta x^4) = 0 = \log a + 5\log x + \lambda o\gamma_4^5\,;$$

$$\therefore \quad -\log x = \frac{\lambda o\gamma_4^5 + \log a}{5} \quad . \quad . \quad . \quad . \quad . \quad (1).$$

The equation of the argument, $\mathfrak{m}_4^5$ of $\lambda o\gamma_4^5$ is $\mathfrak{m}_4^5 = \log(ax^5) - \log(\beta x^4)$

$$\therefore \quad -\log x = \log a - \log \beta - \mathfrak{m} \quad . \quad . \quad . \quad . \quad (2).$$

By equating the values of $\log x$ in (1) and (2) we obtain

$$5\mathfrak{m}_4^5 + \lambda o\gamma_4^5 = 4\log a - 5\log \beta, \text{ or}$$
$$\mathfrak{m}_4^5 + \tfrac{1}{5}\,\lambda o\gamma_4^5 = \tfrac{4}{5}\log a - \log \beta = q \quad . \quad . \quad . \quad . \quad (\text{I}.)$$

This may be considered a formula of reduction for the 5th degree. The character of the solution then is:—The equation $ax^5 + \beta x^4 = 1$ is made dependent on the solution of the transcendental equation $y + \frac{1}{5}\overline{\phi y} = c$, while the latter is soluble by reason that tables of $\overline{\phi y}$ $\left(i.e., \log\left(1 + \frac{1}{10^y}\right)\right)$ have been computed.

The operator is now to look in the table of *Logarithmic Differences*

* If the equation has two real roots, it must have three.

for values of $\mathfrak{m}$ and $\lambda o\gamma$ which satisfy the relation (I.). The nearest tabular places may be taken out at once by a mental computation identical with that which we perform in long division * in finding the successive figures of the quotient. This slight mental effort might, however, be saved by preparing a four-place table, in which the value of $\mathfrak{m}$ would be given to the argument q for the 3rd, 4th, and 5th degrees respectively. The fifth figure would then be interpolated; as to which see note at the end of this paper. $\lambda o\gamma$ would then be found by the existing tables from $\mathfrak{m}$ as argument. The proportional parts are to be subtracted, because $\lambda o\gamma$ diminishes as $\mathfrak{m}$ increases.

The quantities $\lambda o\gamma_4^5$ and $\mathfrak{m}_4^5$ being thus determined, x is immediately found from (1) or (2); but the possible error in the last decimal place, due to the imperfections of the tables, is avoided by using the form (1), in which the sum of the quantities has to be divided by 5 for an equation of the 5th degree, in order to obtain $\log x$, which is the required solution.

(2) From the given equation, $ax^5 + \beta x^4 = 1$, we also have from (b)

$$\log\{ax^5 + \beta x^4\} = 0 = \log(\beta x^4) + \lambda o\gamma_5^4, \text{ whence}$$

$$-\log x = \frac{\lambda o\gamma_5^4 + \log\beta}{4}; \quad \ldots\ldots \quad (1).$$

$$-\log x = \mathfrak{m}_5^4 + \log a - \log\beta \quad \ldots\ldots \quad (2).$$

$$\left.\begin{aligned} \therefore \quad 5\log\beta - 4\log a &= 4\,\mathfrak{m}_5^4 - \lambda o\gamma_5^4; \\ \mathfrak{m}_5^4 - \tfrac{1}{4}\lambda o\gamma_5^4 &= \tfrac{5}{4}\log\beta - \log a = q. \end{aligned}\right\} \text{ Formula of Reduction II.}$$

If the result of substituting the numerical values of $\log\beta$ and $\log a$ in the formula of reduction II. is to make q negative, the formula is to be used with signs changed.

(3) For the equation $ax^5 - \beta x^4 = 1$, we have

$$\log\{ax^5 - \beta x^4\} = 0 = \log(ax^5) - \lambda o\gamma_4^5;$$

$$\therefore \quad \log x = \frac{\lambda o\gamma_4^5 - \log a}{5} \quad \ldots\ldots \quad (1).$$

Also, $\log x = \mathfrak{m}_4^5 + \log\beta - \log a; \quad \ldots\ldots \quad (2).$

whence

$$\left.\begin{aligned} 5\mathfrak{m}_4^5 - \lambda o\gamma_4^5 &= 4\log a - 5\log\beta; \\ \mathfrak{m}_4^5 - \tfrac{1}{5}\lambda o\gamma_4^5 &= \tfrac{4}{5}\log a - \log\beta = q. \end{aligned}\right\} \text{ Formula of Reduction III.}$$

* In long division, when we mentally find a figure of the quotient, we solve an equation of the form $y = ax + z$.

(4) If βx^4 be $> ax^5$, we have

$$\log\{\beta x^4 - ax^5\} = 0 = \log\beta + \log(x^4) - \lambda o\gamma^4_5\,;$$

$$\therefore \quad \log x = \frac{\lambda o\gamma^4_5 - \log\beta}{4} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1).$$

Also, $$\log x = \log\beta - \log a - \mathfrak{m}^4_5\,; \quad . \quad . \quad . \quad . \qquad (2).$$

whence
$$\left.\begin{aligned} 4\mathfrak{m}^4_5 + \lambda o\gamma^4_5 &= 5\log\beta - 4\log a\,; \\ \mathfrak{m}^4_5 + \tfrac{1}{4}\lambda o\gamma^4_5 &= \tfrac{5}{4}\log\beta - \log a = q. \end{aligned}\right\}\ \text{Formula of Reduction IV.}$$

In using formulæ of reduction III. and IV. (1) the quantities $\lambda o\gamma$ and $\mathfrak{m}$ are to be taken from the table of *Logarithms of Subtraction.* (2) If the result of inserting the numerical values of $\log a$ and $\log\beta$ is to make q negative, the formula is to be used with signs changed.

For the equation of terms of contrary signs, a root can always be obtained from either of the formulæ III. and IV.

Example (1).

$$x^5 + \tfrac{1}{4}x^4 = 36\,; \qquad \text{or} \qquad \cdot 02\dot{7}x^5 + \cdot 0069\dot{4}x^4 = 1.$$

The first *formula of reduction* is the one to be used, or

$$4\log a - 5\log\beta = 5\,\mathfrak{m}^5_4 + \lambda o\gamma^5_4.$$

$a = \cdot 02\dot{7}\,;\ \beta = \cdot 0069\dot{4}.$

(I.)

$\log a = \log \cdot 02\dot{7} =$	$\bar{2}\cdot 4436977\,;$	$\log\beta =$	$\bar{3}\cdot 8416377$
$4\log a$	$\bar{7}\cdot 7747908\,;$	$5\log\beta$	$\overline{11}\cdot 2081885$
Subtracting $5\log\beta$	$\overline{11}\cdot 2081885$		
$4\log a - 5\log\beta = q =$	$4\cdot 5666023 = 5\,\mathfrak{m}^5_4 + \lambda o\gamma^5_4$	. . .	(a)

Looking down the table of *Logarithms of Addition* for the nearest values of $\mathfrak{m}$ and $\lambda o\gamma$ corresponding to $5\,\mathfrak{m} + \lambda o\gamma = 4\cdot 56$ or $\mathfrak{m} + \frac{1}{5}\lambda o\gamma = 0\cdot 91$ we find,

$\mathfrak{m} =$	$0\cdot 90300$	$\lambda o\gamma =$	$0\cdot 0511625$
Prop. part,	9	Prop. part,	-98
$\mathfrak{m}^5_4$	$\cdot 90309$	$\lambda o\gamma^5_4$	$\cdot 0511527$
		Add $5\mathfrak{m}^5_4$	$4\cdot 51545$
			$4\cdot 56660 = q$, agreeing with (a).

II. The *Equation of Solution* is, as above (1), $-\log x = \frac{1}{5}\{\lambda o\gamma^5_4 + \log a\}$

$$\log a = \bar{2}\cdot44369$$
$$\lambda o\gamma^5_4, \quad \cdot05116$$
$$\bar{2}\cdot49485$$

$$\tfrac{1}{5}\{\log a + \lambda o\gamma\} = \bar{1}\cdot69897 = \log\frac{1}{x}\ ; \text{ whence } \frac{1}{x} = \cdot5\ ;\ x = 2.$$

This value evidently satisfies the given equation.

Example (2).

$$x^5 - \tfrac{1}{4}x^4 = 10\ ; \qquad \text{or} \qquad \cdot10x^5 - \cdot025x^4 = 1.$$

The third *formula of reduction* is the one to be used, or

$$4\log a - 5\log\beta = 5\mathfrak{m}^5_4 - \lambda o\gamma^5_4 \qquad [a = \cdot10\ ;\ \beta = \cdot025.$$

I.

$$\log a = \log\cdot10 = \bar{1}\cdot0 \quad ; \qquad \log\beta = \log\cdot025 = \bar{2}\cdot39794$$
$$4\log a, \quad \bar{4}\cdot0000 \qquad 5\log\beta \quad \bar{9}\cdot9897$$
$$\text{Subtracting } 5\log\beta \quad \bar{9}\cdot9897$$

$$4\log a - 5\log\beta = q = 4\cdot0103 = 5\mathfrak{m} - \lambda o\gamma^5_4 \quad . \ . \ . \ . \ . \ . \ . \ . \quad (a).$$

The nearest tabular values in the table of *Logarithms of Subtraction* are

$$\mathfrak{m}^5_4 \quad 0\cdot81640 \qquad \lambda o\gamma^5_4\ \cdot07192$$
$$\text{Pp.} \quad 4 \qquad \text{The proportional part is insensible}$$
$$0\cdot81644 \qquad \text{in the result.}$$
$$5\mathfrak{m}^5_4 \quad 4\cdot08220$$
$$\text{Subtracting } \lambda o\gamma^5_4 \quad \cdot07192$$

$5\mathfrak{m}^5_4 - \lambda o\gamma^5_4$, 4·01028, which agrees with the value given by (*a*).

II. *The Equation of Solution is*, $\log x = \frac{1}{5}\{\lambda o\gamma^5_4 - \log a\}$.

		VERIFICATION.		
$\lambda o\gamma^5_4$,	0·07192			
$-\log a$,	$-\bar{1}\cdot0$			
$\lambda o\gamma^5_4 - \log a$,	1·07192		$x =$	1·63825
$\frac{1}{5}\{\lambda o\gamma^5_4 - \log a\} = \log x$	·214384		x^5	11·8010
Also $\log x^5$	1·07193	x^4, 7·2035 ; $\frac{1}{4}$, 1·8009	$\frac{1}{4}x^4$	1·8009
$\log x^4$	0·85754			10·0001

Thus the value of the equation resulting from the value found for x is correct within the limits of accuracy attainable in logarithmic computation.

Solution of the Three-Term Equation of the n*th degree.*

$$\alpha x^n \pm \beta x^{n-p} = 1\,.$$

I. *Where the two terms of* x *are both positive*, we have the two forms corresponding respectively to

$$\log\{\alpha x^n + \beta x^{n-p}\} = \left\{ \begin{array}{l} = \log(\alpha x^n) + \lambda o\gamma^n_{n-p} \\ = \log(\beta x^{n-p}) + \lambda o\gamma^{n-p}_p \end{array} \right\}$$

Case (1). $\operatorname{Log}\{\alpha x^n + \beta x^{n-p}\} = 0 = \log \alpha + n\log x + \lambda o\gamma^n_{n-p}\,;$

$$\therefore \quad -\log x = \frac{\lambda o\gamma^n_{n-p} + \log \alpha}{n} \quad . \quad . \quad . \quad . \quad . \quad (1).$$

Also

$$\log(\alpha x^n) - \log(\beta x^{n-p}) = \mathfrak{m}^n_{n-p}\,;$$

$$\therefore \quad -\log x = \log \alpha - \log \beta - \mathfrak{m}^n_{n-p} \quad . \quad . \quad . \quad . \quad (2).$$

Equating (1) and (2),

$$p\,\lambda o\gamma^n_{n-p} + p\log \alpha = n\log \alpha - n\log \beta - n\mathfrak{m}^n_{n-p}\,;$$

whence

$$\mathfrak{m}^n_{n-p} + \left(\frac{p}{n}\right)\lambda o\gamma^n_{n-p} = \left(\frac{n-p}{n}\right)\log \alpha - \log \beta = q \quad . \quad . \quad (A)$$

For the indices n and $(n-1)$, this becomes

$$\mathfrak{m}^n_{n-1} + \left(\frac{1}{n}\right)\lambda o\gamma^n_{n-1} = \left(\frac{n-1}{n}\right)\log \alpha - \log \beta . \left\{ \begin{array}{c} \text{Formula of} \\ \text{Reduction} \end{array} \right\} (A_1).$$

Case (2). *Where* βx^{n-p} *is the greater of the two terms*, in the positive equation,

$$\log\{\alpha x^n + \beta x^{n-p}\} = 0 = \log \beta + (n-p)\log x + \lambda o\gamma^{n-p}_p\,;$$

$$\therefore \quad -\log x = \frac{\log \beta + \lambda o\gamma^{n-p}_n}{(n-p)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

Also

$$-\log x = \frac{\mathfrak{m}^{n-p}_n + \log \alpha - \log \beta}{p}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

whence,

$$\mathfrak{m}^{n-p}_n - \left(\frac{p}{n-p}\right)\lambda o\gamma^{n-p}_n = \left(\frac{n}{n-p}\right)\log \beta - \log \alpha = q . \left\{ \begin{array}{c} \text{Formula of} \\ \text{Reduction} \end{array} \right\} (B).$$

For the indices n and $(n-1)$ this becomes

$$\mathfrak{m}^{n-p}_n - \frac{1}{n-1}\,\lambda o\gamma^{n-1}_n = \frac{n}{n-1}\log \beta - \log \alpha \quad . \quad . \quad . \quad (B_1).$$

II. *Where in the given equation the terms of* x *have contrary signs, or*

$$\alpha x^n - \beta x^{n-p} = 1 .$$

CASE (3). $\text{Log}\{\alpha x^n - \beta x^{n-p}\} = 0 = \log \alpha + n \log x - \lambda o \gamma^n_{n-p}$;

$$\therefore \quad \log x = \frac{\lambda o \gamma^n_{n-p} - \log \alpha}{n} \quad . \quad . \quad . \quad . \quad . \quad (1).$$

Also,

$$\log x = \frac{\mathfrak{m}^n_{n-p} + \log \beta - \log \alpha}{p} ; \quad . \quad . \quad . \quad (2).$$

whence,

$$\mathfrak{m}^n_{n-p} - \left(\frac{p}{n}\right)\lambda o \gamma^n_{n-p} = \left(\frac{n-p}{n}\right)\log \alpha - \log \beta = q. \left\{\begin{array}{c}\text{Formula of} \\ \text{Reduction}\end{array}\right\} \quad (C).$$

For the indices n and $(n-1)$, this becomes

$$\mathfrak{m}^n_{n-1} - \left(\frac{1}{n}\right)\lambda o \gamma^n_{n-1} = \left(\frac{n-1}{n}\right)\log \alpha - \log \beta = q \quad . \quad . \quad (C_1).$$

The formula (C) is to be used with signs changed when necessary to make q positive.

The quantities $\mathfrak{m}$ and $\lambda o \gamma$ are found from the table of *Logarithms of Subtraction.* These observations apply also to formula (D).

CASE (4). *If* βx^{n-p} *be the greater of the two terms,* we find

$$\mathfrak{m}^{n-p}_n + \left(\frac{1}{n-p}\right)\lambda o \gamma^{n-p}_n = \left(\frac{n}{n-p}\right)\log \beta - \log \alpha = q. \left\{\begin{array}{c}\text{Formula of} \\ \text{Reduction}\end{array}\right\} (D).$$

For the indices $n-1$ and n, this becomes

$$\mathfrak{m}^{n-1}_n + \left(\frac{1}{n-1}\right)\lambda o \gamma^{n-1}_n = \left(\frac{n}{n-1}\right)\log \beta - \log \alpha = q \quad . \quad (D_1).$$

It is evident that the solutions given for the quintic equation are only particular cases of the three-term equation of the nth degree.

Example—

$$x^7 + 3x^4 = 1 . \qquad \left[n = 7 ; \ \frac{p}{n-p} = \tfrac{3}{4} ; \ \frac{n}{n-p} = \tfrac{7}{4}\right.$$

$$\alpha = 1 ; \ \beta = 3 .$$

As x is evidently fractional, the term $3x^4$ is greater than x^7, and we must use the formula of reduction (B), or

$$\tfrac{7}{4}\log 3 - \log (1) = \mathfrak{m}^4_7 - \tfrac{3}{4}\lambda o \gamma^4_7$$

I. $\text{Log}\, \alpha = 0$; $\log \beta = \log 3 = 0{\cdot}4771213$

$q = \tfrac{7}{4}\log \beta =$ $\underline{\underline{0{\cdot}8349623}} = \mathfrak{m}^4_7 - \tfrac{3}{4}\lambda o \gamma^4_7 \quad . \quad . \quad (a).$

The nearest tabular values of $\mathfrak{m}$ and $\lambda o\gamma$, after applying the proportional parts, are

$$\begin{array}{lll} \mathfrak{m}_7^4 & = 0{\cdot}875680 & \qquad\qquad \lambda o\gamma_7^4 = {\cdot}054285 \\ -\frac{3}{4}\lambda o\gamma_7^4, & \underline{0{\cdot}040714} & \\ q & = \underline{\underline{0{\cdot}834966}}, & \text{which agrees with the value found above } (a). \end{array}$$

II. The equation of solution is

$$-\log x = \tfrac{1}{4}\{\log 3 + \lambda o\gamma_7^4\}$$

$\log\beta = \log 3 =$	$0{\cdot}47712$	**Verification.**	
$\lambda o\gamma_7^4,$	$\underline{0{\cdot}05428}$	$\log x,$	$\underline{\bar{1}{\cdot}86715}$
sum,	$0{\cdot}53140$	$\log x^4,$	$\bar{1}{\cdot}46860$
$\frac{1}{4} = -\log x,$	$\underline{\underline{0{\cdot}13285}}$	$\log 3,$	$\underline{\underline{0{\cdot}47712}}$
$\log x,$	$\underline{\bar{1}{\cdot}86715}$	$\log 3x^4,\ \bar{1}{\cdot}94572$;	$3x^4 = {\cdot}88250$
$x =$	$\underline{\underline{0{\cdot}736461}}$	$\log x^7,\ \bar{1}{\cdot}07005$;	$x^7 = \underline{{\cdot}11750}$
		$x^7 + 3x^4 =$	$\underline{\underline{1{\cdot}00000}}$

Thus the value of the equation resulting from the insertion of the value found for x is correct to the last place of decimals.

Apparently the method of logarithmic differences is incapable of extension to equations of more than three terms.

Note as to Mode of finding $\lambda o\gamma$ and $\mathfrak{m}$ from the Tables by Interpolation.

Supposing values of $\mathfrak{m}$ and $\lambda o\gamma$ to be taken out by inspection for the three highest figures or decimal places (which can easily be done mentally), we wish to find these quantities to four and eventually to five places.

Let $\mathfrak{m}''$, λ'' be the nearest values of these quantities in three figures, and let q'' be the approximate value of q resulting from the insertion of $\mathfrak{m}''$, λ'' in the formula of reduction. Let $\mathfrak{m}'$, λ', q' be the corresponding values in four decimal places. The relation between the quantities is of the form $\mathfrak{m} \pm a\lambda = q$.

Then since λ diminishes as $\mathfrak{m}$ increases, we have

$$\mathfrak{m}' - \mathfrak{m}'' \mp a(\lambda' - \lambda'') = q' - q''; \text{ or } \delta q = \delta\mathfrak{m} \mp a(\delta\lambda) \quad . \quad (1).$$

In the table the 3rd figure of the argument $\mathfrak{m}$ is changed at each line; but the 3rd figure of λ changes much more slowly, and the

rate of change is nearly constant through a whole page, or $\frac{\delta^2 \mathfrak{m}}{\delta^2 \lambda}$ is extremely small.

We may therefore proceed as follows:—Taking the upper sign in (1) we first find q'' (or value of q which results from inserting the three figure values of $\mathfrak{m}$ and λ in the formula of reduction); and subtract this from the known or true value of q (as found from the constants) to get δq. Then $\delta q = \delta \mathfrak{m} - (\delta\lambda)a = (s \pm 1)a\delta\lambda$, where s is the *number of lines* through which the value of the 3rd figure of λ is unchanged $\therefore$ $\delta\lambda = \frac{\delta q}{a(s \pm 1)}$. This form gives the nearest value of λ in four figures. With this and the corresponding value of $\mathfrak{m}$ in four figures we may find the value of λ to five places by the same form, only that s now represents not the number of lines but the number of horizontal places in the table from one change of the 5th figure of λ to the next change.

This must be checked by finding q from $\lambda o\gamma$ and $\mathfrak{m}$, and noting whether it corresponds with the true value.

By following this method I have generally been able, in a few minutes, to take out the exact values of $\mathfrak{m}$ and $\lambda o\gamma$ corresponding to the known value of q; and while the solution would be *theoretically* more perfect if we had a supplementary table giving values of $\mathfrak{m}$ to the argument q, this is not practically necessary. Under the actual conditions, the equation of three terms can be solved to such number of places as the tables furnish with very little more trouble than is involved in the solution of a quadratic equation.

I have not given examples of the solution of cubic and quartic equations; but it is evident that the solution by logarithmic differences is very much simpler than the methods now in use, while the preliminary reduction to three terms is equally necessary under either process.

Note.—Since this paper was written, my attention was called by Dr Muir to a short paper in Zeuthen's *Danish Journal of Mathematics*, 1880, p. 135, in which the author makes use of Gaussian Logarithms for the solution of the equation $x^n + px + q$. But as that paper is less general in its aim and method than mine, and the solution given involves some unnecessary transformations, I have thought it desirable to publish my paper as read, subject to this explanation.

On the Reflexion-Caustics of Symmetrical Curves. By the Hon. Lord M'Laren. (With Two Plates.)

(Read April 7, 1890.)

The caustics here considered are those resulting from the reflexion of systems of parallel rays. The subject is purely geometrical. The caustic is considered as a derived curve, and is defined as the envelope of a system of lines drawn from the primary curve, whose inclination to a given line is double the inclination of the normal at the incident point.

It is comparatively easy to find an expression for a caustic in mixed cöordinates. But, where the cöordinates of the primary curve have to be eliminated, the problem becomes more difficult, and unless this condition be fulfilled the expression cannot be considered a true analytical solution of the locus. In the present paper, differential and algebraic expressions are first found containing the cöordinates of the focal point corresponding to a point in the primary or reflecting curve, and the cöordinates of the latter are then eliminated between the differential and algebraic equations.

So far as I am aware, very little has been written on the caustics of reflexion for parallel rays.

Professor Cayley's investigations relate to the caustics of converging and diverging pencils, and include refraction-caustics. In the recent paper of Mr Mannheim, published in the *Transactions of the Academia dei Lincei*, the author develops a method of determining a series of points which are the foci of the reflected rays for a given curve or surface; but his method does not furnish the equation of the continuous caustic or locus of focal points which I conceive to be geometrically the only admissible solution. I may add that a method analogous to that here employed was used by Mr Childe, in his *Treatise on Reflected Ray-Surfaces and their Relation to Plane Reflected Caustics*, to determine the caustic by reflexion for a radiant point which is coincident with the pole or origin of polar cöordinates of the reflecting curve. From the examples given by Mr Childe of the application of his method, it is evidently applicable generally to *two-term* polar equations; though the author does not directly announce this limitation, nor does he treat at all

of the caustics by reflexion of pencils of parallel rays. It appears to me that the case investigated by Mr Childe, and the case of parallel rays here treated, embrace all that can be found by the method of auxiliary curves, and my paper may therefore be regarded as complementary to this part of Mr Childe's work, although the auxiliary curves used and the proofs are quite different for the two cases.

The general solution here given (which is essentially geometrical) was suggested by my solution of a particular case, that of the Reflexion-Caustic of the Parabola, which appeared in the Monthly Notices of the Royal Astronomical Society for 1887. The solution is general for the parallel-ray caustic of any reflecting curve which can be expressed as a *two-term* polar equation. The equivalents of the polar equations in Cartesian coördinates are then determined for the curve of any degree n; and it is shown that these include fundamental forms or types of every class of symmetrical algebraic curves, whether these be finite or infinite in extent, central or parabolic. The solution may be held to apply generally to algebraic curves of *perfect symmetry*, if we consider those curves only to have perfect symmetry whose axes of symmetry are separated by equal angular intervals; in other words, curves whose branches are all equal and are symmetrically disposed about a centre.

1. *Differential Equation of a Reflected Ray.*

I begin with the fundamental expressions for the length (L) of a reflected ray, from the incident to the focal point. These are

$$L = \tfrac{1}{2}\rho_1 \cos\iota \quad . \quad . \quad . \quad (1); \qquad L = \tfrac{1}{2}\rho_2 \sec\iota \quad . \quad . \quad . \quad (2),$$

where ρ_1, ρ_2 are respectively the greatest and least radii of curvature at the incident point, and ι is the angle of incidence or reflexion.

For surfaces of revolution, to which these investigations are usually confined, the relation (1) applies to the focus of consecutive reflected rays in a principal plane; and the relation (2) applies to the focus of consecutive rays reflected from a cyclic section of the reflecting surface (see "Theory of Systems of Rays," *Trans. Royal Ir. Ac.*, vol. xv. p. 97). I shall deal only with the form (1) applicable to reflexion in a principal plane, it being evident that the cone of rays represented by (2) has its vertex or focus in the axis of the surface of revolution if the incident pencil is direct, and that if the

pencil is oblique, or inclined to this axis of symmetry, the rays do not intersect at all, except in a principal plane.

To simplify the investigation the pencil of parallel rays will, in the first instance, be supposed to be parallel to the axis of symmetry (X) of the reflecting curve, which is also taken as the axis of polar cöordinates. The solutions are then extended to the case of oblique pencils. In the notation of this paper,

r, θ are the cöordinates of the primary, or reflecting curve;

p, ω are the cöordinates of its pedal;

v, ψ are the cöordinates of its negative pedal;

R, Θ are the cöordinates of the caustic.

x, y; X, Y are rectangular cöordinates of the primary curve and its caustic.

ρ is the radius of curvature of the primary curve, and is the only radius of curvature necessary to be considered.

ι is the angle of incidence or reflexion.

ν is the angle between any radius-vector and the perpendicular from the pole on the tangent.

In the fundamental relation (1) we have a variable origin (the incident point of the particular ray), and a reference line, ρ (the radius of curvature), whose direction varies from point to point of the reflecting curve. The transposition to a definite origin and a fixed reference line (X) may be effected by finding an expression for the difference of position of a point (S) of the reflecting curve and a corresponding point (C) of the caustic, in cöordinates, x, y, of the reflecting curve, and X, Y of the caustic.

Observing that the inclination of the reflected ray to the axis of X is 2ι, we have, evidently, from (1)

$$\left.\begin{aligned} x - \mathrm{X} &= \frac{1}{2}\rho\cos\iota\cdot\cos\overline{2\iota}\,; \\ y - \mathrm{Y} &= \frac{1}{2}\rho\cos\iota\cdot\sin\overline{2\iota}\,. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If p be the perpendicular drawn from the origin to the tangent at any incident point, S_1, and ω be the inclination of p to the axis, then p is parallel to the normal at S; and, as the incident ray is assumed parallel to the axis of reference, we have,

$$\omega = \iota \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3a)$$

Writing ω for ι, and putting for ρ its known value, $\rho = r\dfrac{dr}{dp}$; and for x and y their values, $r\cos\theta$ and $r\sin\theta$—the equations (3) become—

$$\left.\begin{aligned} X &= r\cos\theta - \frac{1}{2}r\cdot\frac{dr}{dp}\cdot\cos\omega\cdot\cos\overline{2\omega}; \\ Y &= r\sin\theta - \frac{1}{2}r\cdot\frac{dr}{dp}\cdot\cos\omega\cdot\sin\overline{2\omega}. \end{aligned}\right\} \quad . \quad . \quad . \quad (4)$$

From these equations it is evident that if p and ω were known in terms of r and θ, one of these pairs of variables might be replaced by the other, and the equations integrated. This condition is fulfilled in the case of all curves which can be expressed in the two-term polar form $\left\{\begin{matrix} a^n = r^n\cdot\cos(m\theta) \\ a^n = r^n\cdot\sec(m\theta) \end{matrix}\right\}$ where m and n are any numbers, integral or fractional.

It is proposed to investigate the caustics of curves expressed in this form, and thereafter to determine their equivalents in x-and-y cöordinates. It will be shown that the equation of the caustic by reflexion is of the form—

$$R = a\cdot\sec^l(p\theta)\cdot\sin(q\theta);$$
$$\text{or,}\qquad R = a\cdot\cos^l(p\theta)\cdot\sin(q\theta).$$

where l, p, q, are given in terms of m and n.

2. *Systems of Derivative Curves.*

In a system of curves of different degrees, each of which is the pedal of the curve preceding it, and the negative-pedal of the curve following it in the series, by a known theorem, all the tangents of corresponding points of the system make equal angles with their respective radius-vectors. If S_m, S_{m-1}, &c. (fig. 1), be corresponding points in such a series of curves, all the angles $OS_m\,S_{m-1}$, $OS_{m-1}\,S_{m-2}$, &c., are equal. As the adjacent angles are right angles, it follows that the triangles are similar, and that all the angles at the centre, ν_m, ν_{m-1}, &c., are equal. In other words, if ν_m be the angle between the radius-vector and the perpendicular on the tangent for any curve; ν_{m+1} the corresponding angle for the negative-pedal, and ν_{m-1} the corresponding angle for the pedal—then

$$\nu_{m+1} = \nu_m = \nu_{m-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

This is a property of plane curves in general, and is not peculiar to the systems of curves which are considered in this paper.

By a known property of polar equations of the forms—

$$r^m = a^m \cos \overline{m\theta}, \quad . \quad . \quad (a), \qquad \text{and} \qquad r^m = a^m \sec \overline{m\theta}, \quad . \quad . \quad (b),$$

we have $\nu = m\theta$ for all values of m, integral or fractional. This may be verified by logarithmic differentiation of the equations. The forms (a) and (b) are the generalised forms of the equations of the primary curves whose reflexion-caustics are to be found.

(1) From (a) the first form of the equation we have

$$p = r\cos\nu = \frac{r^{m+1}}{a^m}\,;\ \therefore\ r^{m+1} = a^m p\,;\ \text{or, } r = a^{\overline{\frac{m}{m-1}}} \cdot p^{\overline{\frac{m}{m+1}}}\,.$$

Substituting for r its value, the equation of the pedal is

$$p^{\overline{\frac{m}{m+1}}} = a^{\overline{\frac{m}{m+1}}} \cos\nu\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

and similarly, we find for the equation of the negative-pedal,

$$v^{\overline{\frac{m}{1-m}}} = a^{\overline{\frac{m}{1-m}}} \cos\nu\,. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

(2) From (b) the second form of the equation of the primary, we find for the equation of the pedal,

$$p^{\overline{\frac{m}{1-m}}} = a^{\overline{\frac{m}{1-m}}} \sec\nu\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c')$$

and for the equation of the negative-pedal,

$$v \cdot ^{\overline{\frac{m}{m+1}}} = a^{m+1} \sec\nu\,. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d')$$

(3) In order to find a geometrical relation from the polar differential expressions, let the quantities r, p, v, and a be reduced to the first power, then from (a), (c), and (d) we have, by reduction and differentiation,

$$r = a \cdot \cos^{\frac{1}{m}}\nu\,;\ p = a \cdot \cos^{\frac{1}{m}+1}(\nu)\,;\ v = a \cdot \cos^{\frac{1}{m}-1}(\nu)\,. \quad . \quad . \quad (e)$$

$$\frac{dr}{dp} = \frac{d(a \cdot \cos^{\frac{1}{m}}\nu)}{d(a \cdot \cos^{\frac{1}{m}+1}\nu)} = \frac{d(\sec^{\frac{1}{m}+1}\nu)}{d(\sec^{\frac{1}{m}}\nu)} = \frac{d(\sec^2\nu)}{d(\sec\nu)} = 2\sec\nu\,. \quad . \quad . \quad . \quad (f)$$

(4) From (b), (c'), and (d') we obtain similarly

$$r = a\sec^{\frac{1}{m}}\nu\,;\; p = a\cdot\sec^{\frac{1}{m}-1}\nu\,;\; v = a\cdot\sec^{\frac{1}{m}+1}\nu \quad . \quad . \quad (e')$$

$$\frac{dr}{dp} = \frac{d(a\cdot\sec^{\frac{1}{m}}\nu)}{d(a\cdot\sec^{\frac{1}{m}-1}\nu)} = \frac{d(\sec^2\nu)}{d(\sec\nu)} = 2\sec\nu \text{ (as before)} \quad . \quad (f')$$

By a known relation between r, p, and the radius of curvature ρ,

$$\rho = r\frac{dr}{dp}\,;\quad \therefore \quad \frac{\rho}{2} = \frac{r}{2}\cdot\frac{dr}{dp} = a\cdot\cos^{\frac{1}{m}-1}\nu = v \text{ (in the first case)}. \quad . \quad (6)$$

Similarly, $\frac{\rho}{2} = a\cdot\sec^{\frac{1}{m}+1}\nu = v$ (also in the second case). . . . (6)

On this property of the system of curves (which appears to have been hitherto unnoticed) the determination of the equation of the caustic is founded.

The property is this:—The semi-radius of curvature of any curve of the system is equal in length to the radius-vector of the negative-pedal, or next higher curve of the system; so that, if we distinguish these radii by suffixes corresponding to the degrees of the respective curves we have

$$\frac{1}{2}\rho_m = r_{m+1}\,.$$

From the two equations (6) we have also, $\frac{\rho}{2} = a\cdot\sec^{\frac{1}{m}}\nu\cdot\sec\nu$ $= r\sec\nu$, where ρ and r are the radius of curvature and radius-vector of the same curve . . . . (6a)

Moreover, since $\nu = m\theta$ we have for all curves of the series $\omega = (m\pm 1)\theta$; $\psi = (m\mp 1)\theta$, where the signs are determined by the geometrical construction. . . . . (6b)

3. *Reflexion-Caustics of Two-Term Polar Curves of the Degree, m.*

CASE 1.—*Equation of the Reflexion-Caustic of the Curve,*

$$r^{\frac{1}{m}} = a^{\frac{1}{m}}\sec\frac{\theta}{m}\,.$$

In this case, the primitive curve is concave to the pole.

The accompanying diagram (fig. 2) is drawn to scale for curves of

the 4th and 5th fractional degrees of r and a; but is applicable to the proof for polar equations of any fractional degree.

OV is the reference line and axis of symmetry of the curves.

SS′ is the primary, or reflecting curve, which may be denoted by ϕ_1; ΣΣ′, its negative pedal, or ϕ_2. Their equations may be written for present purposes in the form

$$r = a \cdot \sec^m \left(\frac{\theta}{m}\right) = a \sec^m \nu ;$$

$$v = a \cdot \sec^{\frac{m+1}{m}} \left(\frac{\psi}{m+1}\right) = a \sec^{\frac{m+1}{m}} \nu . \quad . \quad . \quad . \quad (\mathrm{d}')$$

S is the incident point, and Σ a corresponding point on the negative-pedal, to which radii OS, OΣ are drawn.

SC is the reflected ray.

OP is a perpendicular on the tangent to the primary at S, or radius-vector of its pedal.

The rectangular figure OSΣQ′ being completed, let a circle be described from T, the intersection of diagonals, as cenrte through the four points O, S, Σ, Q; and let QC be drawn perpendicular to the reflected ray, and therefore meeting it at a point C on the circle.

(1) As the figure ΣSOQ is rectangular by construction, $\angle \Sigma OS = \angle QSO$; ΣOS is also analytically $= \angle SOP = \nu$, because they are respectively the angles between radius-vector and perpendicular on tangent of the two curves, ϕ_2 and ϕ_1, of the system (by 5).

∴ $\angle QSO = \angle SOP$; and SQ is parallel to OP the perpendicular on the tangent to SS′ at S.

∴ SQ is a normal to SS′ at S, and being also $= \Sigma O$ or v, it is the semi-radius of curvature $\frac{\rho}{2}$ (by 6).

Also $$SC = SQ \cdot \cos QSC = \frac{\rho}{2} \cdot \cos \iota = \frac{\rho}{2} \cdot \cos \omega . \quad . \quad . \quad (7)$$

∴ C is a point on the caustic (by 2).

(2) To find its cöordinates R and Θ, we have

$$\Theta = \angle SOV - \angle SOC = \angle SOV - \angle SQC = \theta - (90^\circ - \omega),$$

since the points Q and O lie on the circumference of a circle; or, by expressing all the angles in terms of ν,

$$\therefore \Theta = (2m - 1)\nu - \frac{\pi}{2} ,$$

or again, by changing the reference line for the caustic from X to Y,

$$\Theta = (2m - 1)\nu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

(3) Also, $R = OC = O\Sigma \cdot \sin O\Sigma C = O\Sigma \cdot \sin OSC$
$= O\Sigma \cdot \sin(QSC - QSO)\,; \;\; = O\Sigma \cdot \sin(QSC - SO\Sigma)\,;$

$$\therefore R = v \cdot \sin(\omega - \nu) = a \cdot \sec^{m+1}\nu \cdot \sin(m-2)\nu \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

(by taking the value of v from (e').*

If, in this equation, we substitute for ν its value from (8), $\dfrac{\Theta}{2m-1}$, we obtain finally,

$$\left.\begin{aligned} R &= a \cdot \sec^{m+1}\frac{\Theta}{(2m-1)} \cdot \sin\left(\frac{m-2}{2m-1}\Theta\right), \text{ or} \\ R \cdot \cos^{m+1}\frac{\Theta}{2m-1} &= a \sin\left(\frac{m-2}{2m-1}\Theta\right); \quad . \quad . \quad . \end{aligned}\right\} \quad . \quad (10)$$

the equation of the Reflexion-Caustic.

From (9) we see that when $\nu = 0$, $R = 0$, and there is a cusp at the principal focus.

When $\nu = \dfrac{1}{m-2} \cdot \dfrac{\pi}{2}$, $R = r$, and the caustic meets the reflecting curve.

When $\nu = \dfrac{\pi}{2}$, R is infinite.

CASE 2.—*Reflexion-Caustic of the Curve,*

$$r^{\frac{1}{m}} = a^{\frac{1}{m}} \cos\left(\frac{\theta}{m}\right).$$

The primitive curve in this case also is concave to the pole.

(1) In diagram (3) the construction is similar to diagram (2), only the form of the curve is different, and the order of the points Σ, S and P is reversed. The proof is substantially the same as in case 1, as far as equation (7), and C is a point on the caustic.

By (*c*) and (*d*) the equation of the pedal is given by changing m into $\dfrac{m}{m+1}$, and that of the negative-pedal by changing m into $\dfrac{m}{1-m}$.

* The change of the reference line does not affect the value of R, because the coefficient, $\sin(\omega - \nu)$ is a function of the *difference* of two angles.

But these formulæ are not required for the proof, which, as in the preceding case, is purely geometrical.

(2) To determine the cöordinates of R and Θ, we have

$$\Theta = \angle SOV - \angle SOC = \angle SOV - \angle SQC = \theta - (90^\circ - \omega).$$

$$\therefore \Theta = (2m+1)\nu - \frac{\pi}{2},$$

or by changing the reference line to Y; $\Theta = (2m+1)\nu$. . . (11)

(3) Also,

$$\begin{aligned} R = OC = O\Sigma \cdot \sin O\Sigma C = O\Sigma \cdot \sin OSC\,; \\ = O\Sigma \cdot \sin (QSC + QSO) = O\Sigma \cdot \sin (QSC + SO\Sigma)\,; \\ \therefore R = v \cdot \sin (\omega + \nu) = a \cdot \cos^{m-1}\nu \cdot \sin (m+1)\nu + \nu \\ = a \cdot \cos^{m-1}\nu \cdot \sin (m+2)\nu\,. \end{aligned}$$

(by taking the value of v from (*e*).

Substituting for ν its value from (11), $\nu = \left(\frac{\Theta}{2m+1}\right)$, we have finally—

$$\left.\begin{aligned} R = a \cdot \cos^{m-1}\left(\frac{\Theta}{2m+1}\right) \cdot \sin\left(\frac{m+2}{2m+1}\,\Theta\right), \text{ or} \\ R \cdot \sec^{m-1}\left(\frac{\Theta}{2m+1}\right) = a \sin\left(\frac{m+2}{2m+1}\,\Theta\right) \end{aligned}\right\} \quad . \quad (12)$$

It is easily seen that Cases 1 and 2 are closely related. The curve which passes through corresponding points of the series of pedals is a spiral which never attains the centre, having an infinite number of convolutions in both directions.

Cases 3 and 4.—*Reflexion-Caustics of Curves which are Convex to the Pole.*

The equations of these curves are of the forms—

$$r^n = a^n \cdot \sec n\theta\,;$$
$$r^n = a^n \cdot \cos n\theta.$$

In the curves whose equations are in this paper considered, the pole is evidently either a focus or a centre. In the equations with fractional exponents which have been already considered, the curve is generally parabolic and the pole is an interior focus. But if the

exponent, n, is a whole number, the curve is always central (as will be shown in the sequel), and it consists either of infinite branches which are convex to the centre, or of loops or "foliations" passing through the centre. These forms are shown in diagram (4), which has been very carefully drawn to illustrate the proof of the general theorem for the case of curves of integral degrees and integral coefficients of θ.

The curve SS′S′ is a branch of a real curve of the 4th degree (copied from my paper on "Homogeneous Equations"), consisting of four hyperbolic branches symmetrically placed about O as a centre. The four foliations of the pedal pass through the centre O, which is a point of inflexion for these. Only two of the foliations, P, P′, are shown in the diagram. The negative pedal, ΣΣ′, is necessarily parabolic, because only at the point infinity does its tangent become perpendicular to the radius-vector, or asymptote, of the primary curve.

In the figure, S is the incident point.

S, Σ, and P are corresponding points in the three curves, from which radii are drawn to O.

SQ is the normal produced to SQ′ in the opposite direction.*

The values of R and Θ are found as before, attention being paid to the signs in the general expressions (6 b) viz. $\omega = (m \pm 1)\theta$; $\psi = (m \mp 1)\theta$.

To avoid fractional expressions it is convenient to reduce angles to θ instead of ν. It is unnecessary to give a figure for each variety.

For curves of the forms shown in figure (4), the equation of the primary being $r^n = a^n \cos(n\theta)$, that of the caustic is

$$R = a \cdot \cos^{\frac{1-n}{n}}\left(\frac{n\Theta}{2-n}\right) \cdot \sin\frac{(2n-1)\Theta}{2-n}.$$

Case 5.—*Reflexion-Caustic of the Curve* $r^n = a^n \cdot \cos m\theta$.

Where the coefficient of θ has a value different from the exponent of r and a, we can also obtain geometrical solutions or simultaneous

* SC is the reflected ray produced to C′ in the opposite direction. The proof is the same as in Case 1, as far as Equation (7), if we substitute SC′ for SC. Then taking SC = SC′, C is a point on the caustic. It is now evident that the curve should have been treated as a convex reflector. C′ is a point on the true caustic, if the direction of the incident ray be reversed.

values of lines and angles from which points on the caustic may be computed.

These are, in the first instance, obtained in terms of the cöordinates of a fundamental curve, $r^n = a^n \cos(n\theta)$, whose caustic has already been found. The fundamental curve cöordinates r, θ, ω, p, &c., are distinguished by the suffix (1). The cöordinates of the given curve, or primary, and those of its pedal, negative-pedal, and caustic, are distinguished by the suffix (2). These are—

$$r_2, \theta_2; \quad p_2, \omega_2; \quad v_2 \psi_2; \quad R_2 \Theta.$$

(1) I consider those to be corresponding points of the fundamental curve, $\phi_1(r_1, \theta_1)$, and the given primary curve, $\phi_2(r_2, \theta_2)$, in which $r_1 = r_2$; accordingly the suffixes for r are omitted in the proof. Hence for such corresponding points,

$$\cos^{\frac{1}{n}} \overline{n\theta_1} = \cos^{\frac{1}{n}} \overline{m\theta_2}; \quad \theta_2 = \frac{n}{m} \cdot \theta_1; \quad . \quad . \quad . \quad (1)$$

(2) For the perpendicular on tangent (or radius-vector of the pedal), we have $p_2 = r \cos \nu_2$;

$$\therefore \quad p_2 = r^2 \cdot \frac{d\theta_2}{ds} = r^2 \cdot \frac{d\left(\frac{n\theta_1}{m}\right)}{ds} = \frac{n}{m} r^2 \cdot \frac{d\theta_1}{ds};$$

$$p_2 = \frac{n}{m} p_1; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

also,

$$\cos \nu_2 = \frac{p_2}{r} = \frac{np_1}{mr} = \frac{n}{m} \cdot \cos \nu, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

also,

$$\omega_2 = \nu_2 \mp \theta_2 = \nu_2 \mp \frac{n}{m} \theta_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3a)$$

(3) For the radius-vector of the negative-pedal—observing that, by the general law given above (5), $\angle$ between p and $r = \angle$ between r and $v = \nu$—we have by similar right-angled triangles,

$$\frac{p_2}{r} = \frac{r}{v_2}; \therefore v_2 = \frac{r^2}{p_2} = \frac{m}{n} r \sec \overline{m\theta_2} = \frac{m}{n} r \sec \overline{n\theta_1}; \text{ (By 1).}$$

$$\therefore \quad v_2 = \frac{mv_1}{n}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

For the angular coefficient,

$$\psi_2 = \nu_2 \pm \theta_2 = \nu_2 \pm \frac{n}{m} \theta_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4a)$$

(4) Comparing (4) with (2) we see that the ratios $\frac{p_1}{v_1}$, $\frac{p_2}{v_2}$ are equal. But, as before found, see Title (2), Equation (6), $v_1 = \frac{\rho_1}{2}$;

$$\therefore \text{ finally,} \qquad v_2 = \frac{1}{2}\rho_2 . \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

It is thus proved that for all curves of the form,

$$r^n = a^n \cos \overline{m\theta},$$

where the exponent may be either integral or fractional, the semi-radius of curvature of the primary curve is equal to the radius-vector of the negative-pedal.*

(5) It results from the value last found that all the geometrical relations deduced from the diagrams for the fundamental curves are true for similarly constructed figures applicable to the curves here considered, where m and n have different values.

If we take a system of curves in the form of the figure of diagram 4, we find for R_2 and Θ_2—

From the relation,

$$\angle SOC' = \angle SQ'C' = 90° - QSC = 90° - \omega_2;$$

$$\therefore \qquad \Theta_2 = \theta_2 + SOC' = \theta_2 + \frac{\pi}{2} - \omega_2; \qquad . \quad . \quad . \quad . \qquad (6)$$

From the relation, $\angle CO\Sigma = \angle SO\Sigma - SOC' = \nu_2 + \omega_2 - \frac{\pi}{2}$,

$$\therefore \qquad R_2 = O\Sigma \cdot \cos CO\Sigma = v_2 \cdot \sin(\nu + \omega_2) \qquad . \quad . \quad . \quad . \qquad (7)$$

From these simultaneous equations, a point R_2, Θ_2 on the caustic may be found corresponding to any point on the primary; and from (3) and (4), points on the positive and negative pedals may be similarly determined.

Lastly, It is evident, by inspection of the diagrams, that all circles described on a semi-radius of curvature, as diameter, pass through the pole, which pole is either a centre or (in the case of parabolic curves) a focus.

This curious geometrical property is known to be a property of parabolas of the 2nd degree, and from it, if four points be given, the focus of the parabola may be found. I am not aware that the

* The proof probably admits of extension to the form $r^n = a^n \cos m\theta \cdot \cos p\theta$, but this has not been completely investigated.

extension of this theorem to all two-term polar equations has been previously given.

By means of Formula 10, the equations of the following curves with their caustics may be written:—

	Reflecting Curve.	Caustic.
$m = 2$	$r = a\cdot\sec^{2}(\frac{1}{2}\theta)$	$R = 0\cdot$(Parabola).
$m = 3$	$r = a\cdot\sec^{3}(\frac{1}{3}\theta)$	$R = a\cdot\sec^{4}(\frac{1}{5}\theta)\cdot\sin(\frac{1}{5}\theta)$.
$m = 4$	$r = a\cdot\sec^{4}(\frac{1}{4}\theta)$	$R = a\cdot\sec^{5}(\frac{1}{7}\theta)\cdot\sin(\frac{2}{7}\theta)$.
$m = 5$	$r = a\cdot\sec^{5}(\frac{1}{5}\theta)$	$R = a\cdot\sec^{6}(\frac{1}{9}\theta)\cdot\sin(\frac{1}{3}\theta)$.
$m = 8$	$r = a\cdot\sec^{8}(\frac{1}{8}\theta)$	$R = a\cdot\sec^{9}(\frac{1}{15}\theta)\cdot\sin(\frac{2}{5}\theta)$.
$m = 11$	$r = a\cdot\sec^{11}(\frac{1}{11}\theta)$	$R = a\cdot\sec^{12}(\frac{1}{21}\theta)\cdot\sin(\frac{3}{7}\theta)$.
$m = 14$	$r = a\cdot\sec^{14}(\frac{1}{14}\theta)$	$R = a\cdot\sec^{15}(\frac{1}{27}\theta)\cdot\sin(\frac{4}{9}\theta)$.
$m = 17$	$r = a\cdot\sec^{17}(\frac{1}{17}\theta)$	$R = a\cdot\sec^{18}(\frac{1}{33}\theta)\cdot\sin(\frac{5}{11}\theta)$.
$m = 20$	$r = a\cdot\sec^{20}(\frac{1}{20}\theta)$	$R = a\cdot\sec^{21}(\frac{1}{39}\theta)\cdot\sin(\frac{6}{13}\theta)$.

For all values of m exceeding 5, the coefficients of θ follow a regular law. If $m = 3n + 5$, the first coefficient of θ is $\frac{1}{6n+9}$, and the second coefficient of θ is $\frac{n+1}{2n+3}$.

Similar results might be given for the caustics comprised in Formula 12, and of course in both cases, also for fractional indices of the secants corresponding to integer indices of r and a in the usual form of the equation.

Case 6.—*Caustics of Oblique Pencils.*

The case of a pencil of parallel rays inclined to the axis of the reflecting curve may be next considered.

Let β be the inclination of the pencil to the axis of symmetry, then the inclination of the reflected ray to the normal is $\omega + \beta$, and the distance of the new focal point, C′, is $\frac{\rho}{2}\cdot\cos(\omega + \beta)$.

By inspection of diagram 2, it is evident that as OC′S is a right angle, the new point C′ lies on the circle ΣSOQ, but is nearer the reflecting curve or further from it, according as the incident ray is inclined from the axis or towards it.

In going over the proof, if we allow for the quantity β, we shall find for Θ in the caustic the value $(2m - 1)\nu \pm \beta$; and for R the value $r' \sin(m - 2)\nu \pm \beta$.

The equation of the caustic of oblique pencils (CASE 1) is accordingly

$$R = a \sec^{m+1}\left(\frac{\Theta \pm \beta}{2m-1}\right) \times \sin\frac{(m-2)\Theta + \beta}{(2m-1)} \quad . \quad . \quad (13).$$

In *the parabola* we have from this equation

$$R = a \cdot \sec^3\left(\frac{\theta}{3} + \frac{\beta}{3}\right) \cdot \sin\frac{\beta}{3}\,; \text{ or } R = a \cdot \sin\gamma \cdot \sec^3\left(\frac{\theta}{3} + \gamma\right).$$

By taking a new reference line, making $\angle = \gamma$ with the old, this reduces to

$$R = \sec^3\frac{\theta}{3},$$

which is the equation found by an independent geometrical proof in the paper read to the Royal Astronomical Society, referred to in the introductory paragraph of this paper.

We now see that this is only a particular case of the form (12) of the general equation of the reflexion-caustics of polar curves.

4. *Locus of the Field of View in the Optical Caustic.*

In the preceding paragraph it is pointed out that for a given point S on the reflecting surface, all the focal points C, C′, &c., for pencils of different inclinations lie on the same circle ΣSOQ.

If S_0 be the vertex of the reflecting curve, the series of points C, C′, &c., are the principal foci for different pencils (*e.g.*, stars in the telescopic field), and these all lie on a circle in a principal plane whose diameter is the principal focal length. For a reflecting surface of revolution the field of view is accordingly the surface of a sphere whose diameter is the focal length.

From a note communicated to me by Professor Cayley, and quoted in the above-mentioned paper, it would appear that this is a property of all reflecting surfaces. It may now be added that if any other point S_1 on the reflecting surface be taken, the foci for all pencils reflected at this point in a principal plane also lie on a circle whose diameter is half the radius of curvature at the point.

This property is not confined to the polar curves here investigated, because it results from the general equation for the length of the reflected ray as given by Sir William Hamilton in the paper above referred to. This circle corresponds to the generating circle of the

epicycloid which is the caustic of the circle of curvature at the given point; and the caustic of any reflecting curve is evidently the envelope of all the epicycloidal caustics of the circles of curvature.

5. *Equivalents in Cartesian Cöordinates of the Polar Curves whose Caustics have been found.*

For polar curves having integral indices and coefficients n and m, the equivalents in rectangular cöordinates are easily found. Cos $m\theta$, when expanded in terms of cos θ, is always a homogeneous expression in sin θ and cos θ, provided that m is an integer, and it is easily proved by the inductive method that, if the expansion be homogeneous for any one value of m, it is also homogeneous for $m+1$, and therefore for any integral value of m. The expansions of $\cos m\theta$ for even numbers from $m=2$ to $m=10$ are as follows:—

$$\text{Cos}\ \overline{2\theta} = \cos^2\theta - \sin^2\theta\,;$$

$$\text{Cos}\ \overline{4\theta} = \cos^4\theta - 6\cos^2\theta\cdot\sin^2\theta + \sin^4\theta\,;$$

$$\text{Cos}\ \overline{6\theta} = \cos^6\theta - 15\cos^4\theta\cdot\sin^2\theta + 15\cos^2\theta\cdot\sin^4\theta - \sin^6\theta\,;$$

$$\text{Cos}\ \overline{8\theta} = \cos^8\theta - 28\cos^6\theta\cdot\sin^2\theta + 70\cos^4\theta\cdot\sin^4\theta - 28\cos^2\theta\cdot\sin^6\theta + \sin^8\theta\,;$$

$$\text{Cos}\ \overline{10\theta} = \cos^{10}\theta - 45\cos^8\theta\cdot\sin^2\theta + 210\cos^6\theta\cdot\sin^4\theta - 210\cos^4\theta\cdot\sin^6\theta + 45\cos^2\theta\cdot\sin^8\theta - \sin^{10}\theta.$$

The law of the expansion is this:—It contains the even terms of the binomial theorem with the sign of every alternate term changed. The expansion of $\sin\overline{m\theta}$ contains the odd terms of the binomial theorem, with the signs of alternate terms changed.

From the values of $\cos m\theta$ above given the equivalents of polar equations for even values of n and m may be written out as follows:—

For equations of the form $r^n = a^n\sec(m\theta)$, or $a^n = r^n\cos(m\theta)$, suppose $n=10$;

$$a^{10} = r^{10}\cos(10\theta).$$
$$= x^{10} - 45x^8y^2 + 210x^6y^4 - 210x^4y^6 + 45x^2y^8 - y^{10}\,; \quad . \quad . \quad . \quad (1)$$

$$a^{10} = r^{10}\cdot\cos(8\theta) = (x^2+y^2)\cdot r^8\cdot\cos(8\theta),$$
$$= (x^2+y^2)\{x^8 - 28x^6y^2 + 70x^4y^4 - 28x^2y^6 + y^8\cdot\}\,; \quad . \quad . \quad . \quad (2)$$

$$a^{10} = r^{10} \cdot \cos(6\theta) = (x^2+y^2)^2 \cdot r^6 \cdot \cos 6\theta,$$
$$= (x^2+y^2)^2\{x^6 - 15x^4y^2 + 15x^2y^4 - y^6\}; \quad \ldots\ldots \quad (3)$$
$$a^{10} = r^{10}\cos(4\theta) = (x^2+y^2)^3\{x^4 - 6x^2y^2 + y^4\}; \quad \ldots\ldots \quad (4)$$
$$a^{10} = r^{10}\cos(2\theta) = (x^2+y^2)^4\{x^2 - y^2\}. \quad \ldots\ldots \quad (5)$$

To these may be added—

$a^{10} = r^{10} \cdot \cos 2\theta \cdot \cos 5\theta$; $a^{10} = r^{10}\cos 2\theta \cdot \cos 4\theta$; $a^{10} = r^{10}\cos 2\theta \cdot \cos 3\theta$; $a^{10} = r^{10}\cos^3(2\theta)$; $10^{10} = r^{10}\cos^3(3\theta)$.

When these x-and-y equations are fully expanded, they are found to be central homogeneous equations of the 10th degree, each having its distinct combination of + and − signs. Numbers (1) to (5) are all hyperbolic curves without inflexions, and having the branches of each curve all equal and all symmetrically disposed about a centre. These characteristics evidently apply to all curves of even integral degrees so obtained.

In (1) there are 10 asymptotes, with angular intervals of 18°, and 10 real branches.

In (2) there are 8 asymptotes, with angular intervals of $22\frac{1}{2}$°, and 8 real branches.

In (3), (4), and (5) the number of asymptotes and branches are respectively 6, 4, and 2, and the angular intervals are 30°, 45°, and 90°, the circle being always equally divided. The curves of the equations containing two cosines have not been examined. The above are all the symmetrical homogeneous hyperbolic curves that can be formed in the 10th degree, and for any degree the number of such curves is evidently $n/2$. They are all curves of "perfect symmetry" in the sense defined in the introductory paragraph.

If m exceed n, as, for example, in the expression $a^{10} = r^{10}\cos(12\theta)$, we obtain by transformation to rectangular cöordinates a homogeneous equation of the second class (so termed in my paper on this subject), *i.e.*, of the form $\phi_n(x,y) = \phi_p(x,y)$, or $\dfrac{\phi_n(x,y)}{\phi_p(x,y)} = 1$. The number of such curves (all traceable) for any degree, $n - p$, is infinite.

For the equations of the form $r^n = a^n \cdot \cos(m\theta)$ or $a^n = r^n \sec(m\theta)$, above considered,

we also obtain homogeneous Cartesian equations of the second

LORD M'LAREN ON REFLECTION-CAUSTICS OF CURVES.

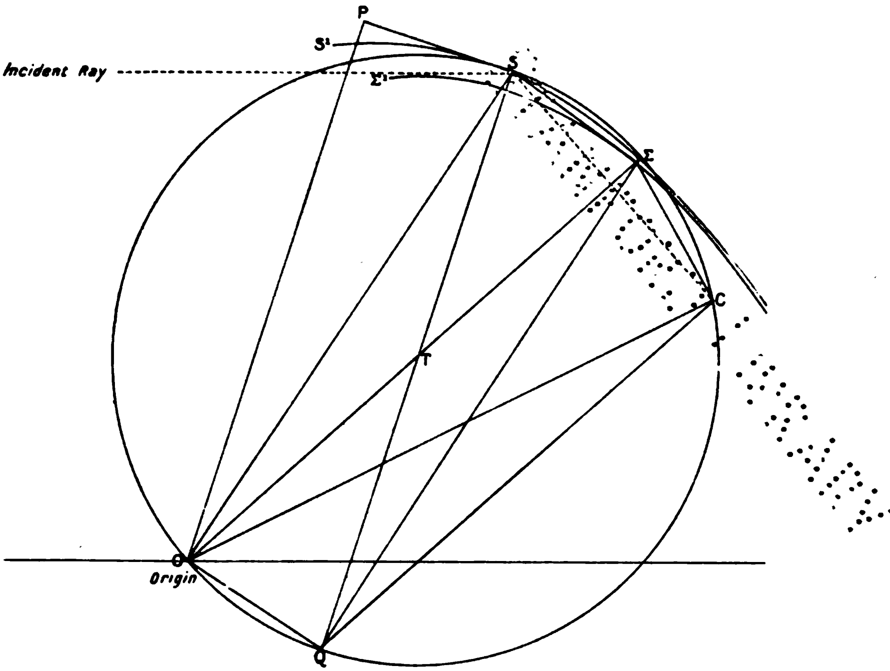
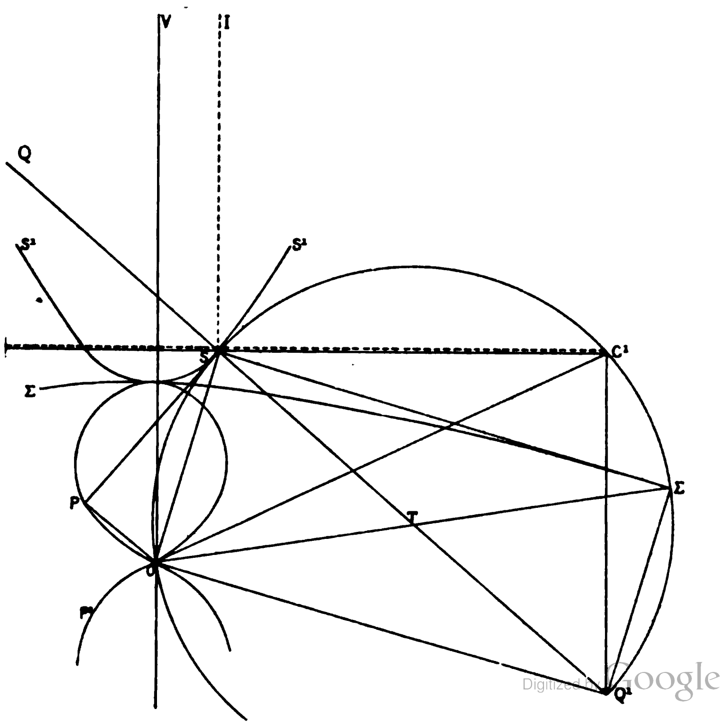


Figure 3.



class. These are of the looped or foliated type. Those of the 6th degree are figured in my paper on homogeneous equations. An infinite number of such curves (all traceable) may be formed for a given degree $(n-p)$ by augmenting the values of n and p.

It is evident that the caustic will not in general be the locus of a homogeneous equation. If we take, for example, the solution in (Equation 10), p. 288,

$$R \cdot \cos^{m+1}\left(\frac{\Theta}{2m-1}\right) = a \cdot \sin\left(\frac{m-2}{2m-1}\Theta\right),$$

the condition that the curve shall be a homogeneous equation of the second class, or of the form $\phi_p(x, y) = \phi_q(x, y)$, is that the coefficients of Θ shall be whole numbers; that is, $\frac{1}{2m-1}$ and $\frac{m-2}{2m-1}$ must be integers, in order that the expansions of the two trigonometrical quantities may each be a homogeneous expression in $\sin\Theta$ and $\cos\Theta$, and so be transformable into homogeneous equivalents in x and y.

Synthesis by Means of Electrolysis.—Part III. Synthesis of n-Dicarbodecahexanic Acid. By Prof. Crum Brown and Dr James Walker.

(Read June 16, 1890.)

(*Abstract.*)

The synthesis of the diethyl ether of this new acid was effected by the electrolysis of a strong aqueous solution of potassium ethyl sebate, $COOC_2H_5(CH_2)_8COOK$. This salt was prepared by adding the calculated quantity of alcoholic potash to an alcoholic solution of diethyl sebate. After some time the potassium ethyl salt began to separate out: the whole was then boiled, allowed to cool, and filtered. What remained on the filter was dissolved in water; the aqueous solution was extracted twice with ether, and then evaporated to a concentration suitable for electrolysis (*cf.* this vol. p. 54).

When the electric current was passed through the cold solution, it rapidly diminished in intensity, and in a few moments ceased to flow altogether. This we found to be due to the fact that the product of electrolysis is solid at the ordinary temperature. Heating

to 50° restored the current. On completion of the electrolysis a colourless oil was seen to float to the surface of the liquid, and this oil on cooling solidified to a white crystalline mass, which was washed several times with water, dried, and analysed. The results of analysis were as follows:—

·1628 gr. substance gave ·4254 gr. CO_2
and ·1687 gr. H_2O

	Found.	Calculated for $C_{22}H_{42}O_4$
C	71·26 per cent.	71·35 per cent.
H	11·51 „	11·35 „

The substance thus corresponds to the formula $C_{22}H_{42}O_4$, showing that the reaction took place in the expected direction, viz.—

$$2C_2H_5COO(CH_2)_8COOK = C_2H_5COO(CH_2)_{16}COOC_2H_5 + 2CO_2 + 2K$$

The acid of which this substance is the diethyl ether we propose to name *n*-dicarbodecahexanic acid. The ether melts at 43°, is practically insoluble in water, soluble in ether, is sparingly soluble in cold alcohol, but when melted mixes freely with hot alcohol. It possesses a faint, somewhat unpleasant smell, and decomposes rapidly when heated to 200°. In order to saponify it, its boiling alcoholic solution should be added gradually to a boiling solution of potash in alcohol. The potassium salt separates out. It is easily soluble in water, giving a soapy solution.

Analysis of the potassium salt gave the following numbers:—

·3565 gr. substance gave ·1253 gr. K_2CO_3

	Found.	Calculated for $C_{18}H_{32}O_4K_2$
K	19·87 per cent.	20·00 per cent.

The calcium, barium, zinc, lead, silver, and copper salts fall out from an aqueous solution of the potassium salt as insoluble, mostly gelatinous, precipitates. The copper salt is green.

The acid itself is precipitated in gelatinous form, but from hot alcohol it separates on cooling in small hard warty masses. It is practically insoluble in water, only slightly soluble in cold alcohol and in ether. Its melting-point is 118°.

The yield of the dicarbodecahexanic ether is over 20 per cent. of the sebacic ether employed.

Synthesis by Means of Electrolysis.—Part IV. Synthesis of Suberic and n-Dicarbododecanic Acids. By Prof. Crum Brown and Dr James Walker.

(Read July 7, 1890.)

(*Abstract.*)

Suberic Acid.—In continuation of our syntheses of dibasic acids by means of electrolysis, we have prepared suberic acid from glutaric acid, and from the former again a new acid, which we term *n*-dicarbododecanic acid.

Potassium ethyl glutarate was electrolysed under the same conditions as are described in our first paper (*Proc. Roy. Soc. Edin.*, 1888–9, p. 54). A colourless oil floated to the top of the solution after completion of the electrolysis. This oil was separated, dried, and heated for some time on the water-bath to drive off any alcohol formed by saponification of the potassium ethyl salt by the potassium (potash) liberated at the negative pole. It was found to boil, not very constantly, between 265° and 275° (uncorrected). Analysis yielded the following results:—

·1137 gr. substance gave ·2602 gr. CO_2
and ·0959 gr. H_2O

	Found.	Calculated for $C_{12}H_{22}O_4$
C	62·41 per cent.	62·61 per cent.
H	9·37 „	9·56 „

On saponification, a white potassium salt was obtained, which could be recrystallised from alcohol. The acid prepared from this by precipitation melted at 138°. Analysis of the potassium salt resulted as follows:—

·2484 gr. gave ·1388 gr. K_2CO_3

	Found.	Calculated for $K_2C_8H_{12}O_4$
K	31·6 per cent.	31·2 per cent.

The yield of ether is about 20 per cent. of the potassium ethyl glutarate employed.

n-Dicarbododecanic Acid.—Potassium ethyl suberate falls out

after a short time when the calculated quantity of moderately strong alcoholic potash is added slowly to a strong alcoholic solution of suberic ether.

A strong solution of this salt, when electrolysed, yields a colourless oil, which floats to the top of the solution. This oil, after being freed from alcohol, solidifies, on standing for twenty-four hours, to a pure white crystalline mass. When dried on porous tile the crystals melt at 27° quite sharply. Analysis:—

·1249 gr. substance gave	·3146 gr. CO_2
	and ·1235 gr. H_2O

	Found.	Calculated for $C_{18}H_{24}O_4$
C	68·69 per cent.	68·79 per cent.
H	10·98 „	10·83 „

The crystals are soluble in alcohol and ether, insoluble in water.

Saponification yielded a potassium salt very soluble in water, and slightly soluble in cold alcohol. A solution of this salt was precipitated by salts of barium, calcium, zinc, silver, and copper, the precipitates being all flocculent and extremely insoluble in water. The copper salt is green. The silver salt is exceedingly soluble in ammonia. The acid prepared from the potassium salt melts at 122°. Analysis of the barium salt yielded the following result:—

·4261 gr. substance gave ·2531 gr. $BaSO_4$

Found.	Calculated for $BaC_{14}H_{24}O_4$
34·92 per cent.	34·86 per cent.

The yield of ether in this case reaches over 20 per cent. of the theoretical value.

Preliminary Experiment on the Thermal Conductivity of Aluminium. By A. Crichton Mitchell, B.Sc.

(Read July 7, 1890.)

The recent improvement in the process of manufacture of pure aluminium, and the consequent lowering in its price, has placed it within the reach of those who desire to make any experiments

requiring large quantities of this substance. It seemed desirable that an investigation of the thermal conductivity of aluminium and its alloys should be made, but before obtaining the necessary bars a smaller bar was obtained from experiments on which a preliminary notion was got of the thermal conductivity; this being necessary in order to determine the most convenient dimensions for the long bars used in Forbes's method of conducting these experiments.

The present note gives the result of this preliminary experiment. The small bar used was furnished by the Aluminium Company, Birmingham, who now manufacture pure aluminium by the Deville-Castner process. Analysis showed that the particular sample used contained nearly 98 per cent. of pure metal, the chief impurity being iron. The method employed for finding the conductivity was substantially that of Forbes, the only difference being that the cooler end of the bar was inserted in a cold bath. The bar used, being 20 inches long and 1 inch square in section, was of such a length that it could be also used for the cooling experiment. Four holes were drilled in the bar, separated by intervals of 3 inches. The thermometers used were those employed in Professor Tait's and my own previous work. The source of heat was simply a Bunsen burner, applied to one end of the bar.

For such a preliminary experiment it is unnecessary to give all the data regarding rates of cooling, values of tangents, &c.; and it will therefore suffice to state simply the result. By these experiments the *thermal* conductivity of aluminium was found to be ·072 when expressed in foot-pound-minute units. This number represents the conductivity at (about) 100° C. The corresponding numbers for other metals are—

Copper, ...	·071
Iron, ...	·0116
German silver, ...	·0078.

Hence the thermal conductivity of aluminium (see *Trans. Roy. Soc. Edin.*, vol. xxxiii. p. 559) is slightly greater than that of the best conducting copper.

Recently (see *Nature*, June 20, 1889), in a lecture delivered at the Royal Institution, London, Sir H. Roscoe stated that Faraday

had found that aluminium conducts heat as well as silver, but I have been unable to find this result of Faraday's, or any reference to it, in his papers.

Note on Electrolytic Conduction. By Robert L. Mond, B.A.

(Read July 10, 1890).

At the suggestion of Professor P. G. Tait, I undertook the further investigation of an experiment, an account of which he communicated to this Society on Monday, the 15th of April 1878.

In this experiment a platinum plate of moderate thickness is inserted between the electrodes of an electrolytic cell, so as to completely fill the cross-section. This platinum plate thus becomes the common electrode of two cells, and on perforating it it is found that a comparatively small hole at once changes its functions to that of an obstacle in one cell.

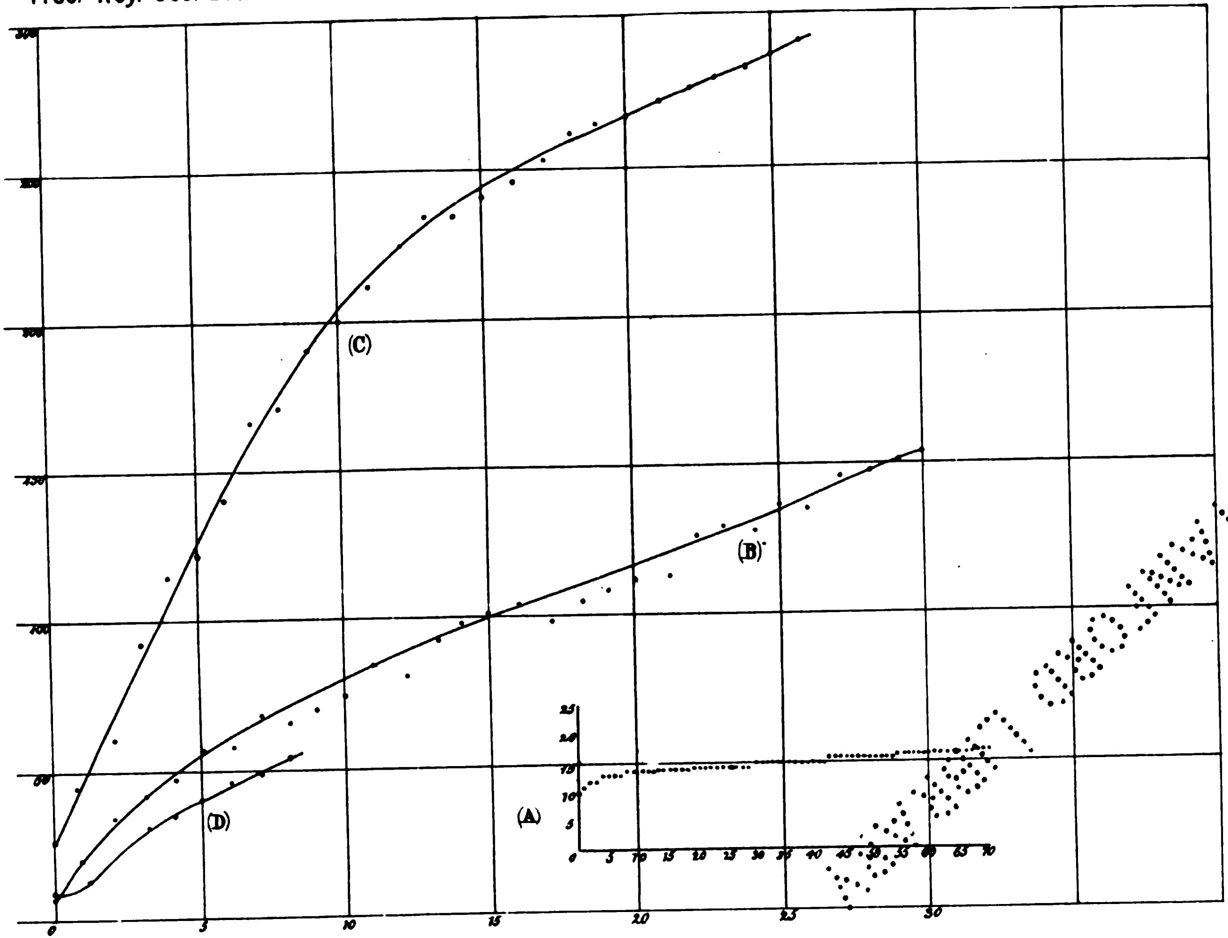
It is interesting to study experimentally as fully as possible the complete chain of events from the first perforation to the total removal of the plate.

The interposition of a platinum plate of the same dimensions as the electrodes (namely, 1 sq. cm.) reduced the strength of the current to about one-half. The plate was then perforated with a fine needle, readings being taken for each perforation.

The strength of the current is observed rapidly to rise, the quantity passing through the plate rapidly and regularly decreasing; a transition period ensues, after which the whole current passes through the holes. The polarisation remains practically constant for the experiment as long as the area of the perforations is small compared to that of the plate.

The results thus obtained were plotted as curves, the deflections of the galvanometer being measured along the axis of ordinates, and the number of holes along the axis of abscissæ.

The first curve (A) was obtained by using one central plate of 1 sq. cm. area, the currents being obtained from storage cells arranged to give 10 volts, and the deflections were measured on a Helmholtz Tangent Galvanometer. Seventy perforations were made, each hole having approximately $\frac{1}{500}$ sq. cm. section.


(A)
(B)
(C)
(D)
0
5
10
15
20
25
30

This curve shows that only the first few points obtained show any rapid rise in current strength, the curve tending to become parallel to axis of abscissæ, which it does when the plate is removed.

In order to more fully elucidate the first points on this curve, three plates, of 1 sq. cm. each, were interposed instead of one, the storage cells being used, arranged in the previous manner, the results for 10 holes being embodied in curve (B), the deflections being those of Sir William Thomson's Reflecting Galvanometer of 22·3 B.A. units resistance, shunted through a resistance of 1 ohm.

Since the mechanical disturbance due to the liberation of gas bubbles vitiated the results obtained, the experiment was repeated with the current from two Bunsen cells whose E. M. F. was about 1·8 volt each; the size of the plates remaining the same, the results for nine holes in each plate are plotted in curve (C).

For the first two holes in the plates the curve is practically a straight line, a great part of the current still passing through the platinum; for the next three holes we observe the transition period, and after that the whole of the current flows through the holes, the current slowly and regularly increasing in strength as the cross-section of the liquid passing through the holes is increased.

This experiment was repeated under the same conditions with much larger plates (having an effective area of 24 compared to 6 sq. cm.). The curve (D) did not give very satisfactory results, it very quickly becoming a straight line.

The experimental difficulties were greater than at first suspected, especially in finding a means for firmly securing the platinum. This was finally achieved by making the whole cell out of a block of paraffin, the platinum being firmly imbedded along the edges in the latter. Great care had also to be taken to keep the surface of the platinum clean and to ensure the electrolyte passing through the holes when made.

In conclusion, I must express my best thanks to Professor Tait for his advice and the facilities placed at my disposal during these and other experiments; likewise to Dr Peddie, for the interest he has taken in my work.

List of West Australian Birds, showing their Geographical Distribution throughout Australia, including Tasmania. By A. J. Campbell, F.L.S. *Communicated by* Rev. James MacGregor, D.D., F.R.S. Edin. (With a Map).

(Read June 16, 1890.)

"There were but few land fowls. We saw none but eagles of the larger sort of birds, but five or six sorts of small birds. The biggest sort of these were not bigger than larks, some no bigger than wrens, all singing with great variety of fine shrill notes; and we saw some of their nests with young ones in them. The water-fowls were ducks (which had young ones now, this being the beginning of the spring in these parts), curlews, galdens, crab-catchers, cormorants, gulls, pelicans, and some water-fowl such as I have not seen anywhere besides." Such are the quaint remarks of the celebrated British navigator, Captain William Dampier, when in Shark's Bay, August 1699, on his second visit to New Holland. This is probably the first recorded note of the avi-fauna of Western Australia, or indeed of any part of Australia, if we except Vlaming, who two years previously introduced into Europe the black swan for the first time. Vlaming in 1697 discovered and named the Swan River (upon which Perth, the capital of Western Australia, stands), on account of the number of these exceedingly handsome birds he found upon that water.

Gilbert, the worthy coadjutor of the immortal John Gould, between the years 1839–42 worked up the bulk of the ornithological notes of Western Australia which we are in possession of, and which are embodied in Gould's *Birds of Australia.* By reason of the number of aboriginals he had to assist him, Gilbert's work was very accurate and complete. On the 28th June 1845, poor Gilbert met a premature death at the hands of treacherous natives during Leichardt's expedition to North Australia. Gould pathetically adds, "and so I lost an able coadjutor and science a devoted follower."

Then George Masters, assistant curator of the Australian Museum, Sydney, on two occasions, and Cockerell on his own

No.	Name of Species.	Vernacular or Colonists' Name.	(a) North-West Australia.	(b) Northern Territory.	(c) North Queensland.	(d) South Queensland.	(e) New South Wales.	(f) Victoria.	(g) South Australia.	(h) Tasmania.	Remarks.
1	*Aquila audax*, Lath.,	Wedge-tailed Eagle (Eaglehawk),	a	b	c	d	e	f	g	h	
2	,, *morphnoides*, Gould,	Little Eagle,	...	b	c	d	e	?	g	...	
3	*Haliaetus leucogaster*, Gm.,	White-bellied Sea-eagle,	a	b	c	d	e	f	g	h	
4	*Haliastur sphenurus*, Vieill.,	Whistling Eagle,	a	b	c	d	e	f	g	...	
5	*Pandion leucocephalus*, Gould,	White-headed Osprey,	a	b	c	d	e	f	g	h	
6	*Falco hypoleucus*, Gould,	Grey Falcon,	...	b	...	d	...	f	g	...	
7	,, *melanogenys*, Gould	Black-cheeked Falcon,	a	...	c	d	e	f	g	h	
8	,, *lunulatus*, Lath.,	White-fronted Falcon,	a	b	c	d	e	f	g	h	
9	*Hieracidea orientalis*, Schl.,	Eastern Brown Hawk,	a	b	c	d	e	f	g	h	
10	,, *berigora*, V. & H.,	Western Brown Hawk	a	...	...	d	...	f	g	...	
11	*Tinnunculus cenchroides*, V. & H.,	Nankeen Kestrel,	a	b	c	d	e	f	g	h	Seen Swan and Champion B. Districts.
12	*Astur approximans*, V. & H.,	Australian Goshawk,	a	...	c	d	e	f	g	h	
13	,, *cruentus*, Gould,	West Australian Goshawk,	a	b	...	...	...	f	g	...	
14	*Accipiter cirrhocephalus*, Vieill.,	Collared Sparrow-Hawk,	a	...	c	d	e	f	g	h	
15	*Gypoictinia melosternon*, Gould,	Black-breasted Buzzard,	a	b	c	d	e	f	g	...	
16	*Milvus affinis*, Gould,	Allied Kite,	a	b	c	d	e	f	g	...	
17	*Lophoictinia isura*, Gould,	Square-tailed Kite,	...	...	...	d	e	f	g	...	
18	*Elanus axillaris*, Lath.,	Black-shouldered Kite,	a	b	c	d	e	f	g	...	
19	*Circus assimilis*, J. & Selb.,	Jardine's Harrier,	a	...	...	d	e	f	g	?	
20	*Strix novæ-hollandiæ*, Steph.,	Masked Owl,	...	b	c	d	e	f	g	...	
21	,, *delicatula*,	Delicate Owl,	a	b	c	d	e	f	g	...	
22	*Ninox connivens*, Lath.,	Winking Owl,	...	b	c	d	e	f	g	...	
23	,, *boobook*, Lath.,	Boobook Owl,	...	b	c	d	e	f	g	...	

In reference to the range of distribution over the other parts of Australia of the West Australian birds, the Table is chiefly the results of observations when on brief excursions to the other colonies during the many years I have been working at the "Oology of Australian Birds." But where personal knowledge of any species did not exist, I have referred to Gould's and Dr Ramsay's tabular lists, so as to render the distribution as complete as possible up to date. For, no doubt, as the various colonies are further explored and opened up, so there will be many extensions of localities, and probably not a few new species or varieties discovered, especially in the far interior and the north-west of our vast island continent.

It occurred to me that the divisional line between west and north-west might be conveniently drawn about the Tropic of Capricorn or the North West Cape. The same imaginary line is also used to subdivide North from South Queensland.

At some future time I hope to furnish a more complete list of the north-west birds of Western Australia, because, knowing the nature of the country, I am sensible that many *hiatus* occur in that column.

There are between 760 and 770 known Australian birds. Rather more than one-third, or about 280, occur in Western Australia (in that portion south of the Tropics), and are distributed as follows:—

[TABLE.

No.	Name of Species.	Vernacular or Colonists' Name.	(a) North-West Australia.	(b) Northern Territory.	(c) North Queensland.	(d) South Queensland.	(e) New South Wales.	(f) Victoria.	(g) South Australia.	(h) Tasmania.	Remarks.
1	*Aquila audax*, Lath.,	Wedge-tailed Eagle (Eaglehawk),	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
2	,, *morphnoides*, Gould,	Little Eagle,	...	*b*	*c*	*d*	*e*	?	*g*	...	
3	*Haliaetus leucogaster*, Gm.,	White-bellied Sea-eagle,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
4	*Haliastur sphenurus*, Vieill.,	Whistling Eagle,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
5	*Pandion leucocephalus*, Gould,	White-headed Osprey,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
6	*Falco hypoleucus*, Gould,	Grey Falcon,	...	*b*	...	*d*	...	*f*	*g*	...	
7	,, *melanogenys*, Gould	Black-cheeked Falcon,	*a*	...	*c*	*d*	*e*	*f*	*g*	*h*	
8	,, *lunulatus*, Lath.,	White-fronted Falcon,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
9	*Hieracidea orientalis*, Schl.,	Eastern Brown Hawk,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
10	,, *berigora*, V. & H.,	Western Brown Hawk	*a*	...	...	*d*	...	*f*	*g*	...	
11	*Tinnunculus cenchroides*, V. & H.,	Nankeen Kestrel,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	Seen Swan and Champion B. Districts.
12	*Astur approximans*, V. & H.,	Australian Goshawk,	*a*	...	*c*	*d*	*e*	*f*	*g*	*h*	
13	,, *cruentus*, Gould,	West Australian Goshawk,	*a*	*b*	...	...	...	*f*	*g*	...	
14	*Accipiter cirrhocephalus*, Vieill.,	Collared Sparrow-Hawk,	*a*	...	*c*	*d*	*e*	*f*	*g*	*h*	
15	*Gypoictinia melosternon*, Gould,	Black-breasted Buzzard,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
16	*Milvus affinis*, Gould,	Allied Kite,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
17	*Lophoictinia isura*, Gould,	Square-tailed Kite,	...	...	...	*d*	*e*	*f*	*g*	...	
18	*Elanus axillaris*, Lath.,	Black-shouldered Kite,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
19	*Circus assimilis*, J. & Selb.,	Jardine's Harrier,	*a*	...	...	*d*	*e*	*f*	*g*	?	
20	*Strix novæ-hollandiæ*, Steph.,	Masked Owl,	...	*b*	*c*	*d*	*e*	*f*	*g*	...	
21	,, *delicatula*,	Delicate Owl,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
22	*Ninox connivens*, Lath.,	Winking Owl,	...	*b*	*c*	*d*	*e*	*f*	*g*	...	
23	,, *boobook*, Lath.,	Boobook Owl,	...	*b*	*c*	*d*	*e*	*f*	*g*	...	

No.	Name of Species.	Vernacular or Colonists' Name.	(a) North-West Australia.	(b) Northern Territory.	(c) North Queensland.	( ) South Queensland.	(e) New South Wales.	(f) Victoria.	(g) South Australia.	(h) Tasmania.	Remarks.
24	*Ninox ocellata*, Hornb. & Jacq.,	... ...	!	*b*	...	...	...	...	...	...	
25	*Ægotheles novæ-hollandiæ*,	Owlet Nightjar,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
26	,, *leucogaster*, Gould,	White-bellied Owlet Nightjar,	*a*	*b*	*c*	*d*	...	...	...	...	
27	*Podargus brachypterus*, Gould,	Short-winged Podargus (Mo-poko),	...	...	...	...	...	...	...	...	
28	*Eurostopodus guttatus*, V. & H.,	Spotted Nightjar,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
29	*Chætura caudacuta*, Lath.,	Spine-tailed Swift,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
30	*Hirundo noexena*, Gould,	Welcome Swallow,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
31	*Petrochelidon nigricans*, Vieill.,	Tree Swallow,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
32	*Lagenoplastes ariel*, Gould,	Fairy Martin,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	Champion Bay Dist. (Cowan).
33	*Cheramœca leucosternum*, Gould,	White-breasted Swallow,	...	...	...	...	*e*	*f*	*g*	...	
34	*Merops ornatus*, Lath.,	Australian Bee-eater (Gold-seeker),	*a*	*b*	*c*	*d*	*e*	*f*		...	
35	*Halcyon sanctus*, V. & H.,	Sacred Kingfisher,	*a*	*b*	*c*	*d*	*e*	*f*		*h*	
36	,, *pyrrhopygius*, Gould,	Red-backed Kingfisher,	*a*	*b*	*c*	*d*	*e*	*f*		...	
37	*Artamus sordidus*, Lath.,	Wood Swallow (Martin),	...	...	*c*	*d*	*e*	*f*		*h*	
38	,, *minor*, Vieill.,	Little Wood Swallow,	*a*	*b*	*c*	*d*	*e*	...	...	...	
39	,, *cinereus*, Vieill.,	Grey-breasted Wood Swallow,	*a*	*b*	...	*d*	...	...	...	...	
40	,, *melanops*, Gould,	Black-faced Wood Swallow,	*a*	*b*	...	...	*e*	...	*g*	...	
41	,, *personatus*, Gould,	Masked Wood Swallow,	...	...	...	*d*	*e*	*f*	*g*	...	
42	*Pardalotus punctatus*, Temm.,	Spotted Diamond Bird,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
43	,, *ornatus*, Temm.,	Striated Diamond Bird,	...	...	*c*	*d*	*e*	*f*	*g*	...	
44	,, *xanthopygius*, M'Coy,	Yellow-rumped Diamond Bird,	...	...	...	...	*e*	*f*	*g*	...	

No.	Name of Species.	Vernacular or Colonists' Name.	(a) North-West Australia.	(b) Northern Territory.	(c) North Queensland.	(d) South Queensland.	(e) New South Wales.	(f) Victoria.	(g) South Australia.	(h) Tasmania.	Remarks.
45	*Strepera plumbea*, Gould,	Leaden-coloured Crow-Shrike (Squeaker),	...	...	...	...	...	...	...	...	
46	*Gymnorhina* (?)	Western Crow-Shrike (Magpie),	*a*	...	...	...	...	...	...	...	If this should prove *G. leuconota*, then columns *e*, *f*, and *g* should be filled in.
47	*Cracticus leucopterus*, Cab.,	White-winged Butcher Bird,	...	...	...	...	...	...	...	...	
48	*Grallina picata*, Lath.,	Pied Grallina (Little Magpie),	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
49	*Graucalus melanops*, Lath.,	Black-faced Cuckoo-Shrike,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
50	*Pteropodocys phasianella*, Gould,	Ground Cuckoo Shrike,	...	...	...	*d*	*e*	*f*	*g*	...	
51	*Lalage tricolor*, Swain.,	White-shouldered Caterpillar-catcher,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
52	*Pachycephalus occidentalus*, Ramsay,	Western Thickhead,	...	...	...	...	...	...	...	...	
53	,, *rufiventris*, Lath.,	Rufus-breasted Thickhead,	...	...	*c*	*d*	*e*	*f*	*g*	...	
54	,, *gilberti*, Gould,	Gilbert's Thickhead,	...	...	...	...	*e*	*f*	*g*	...	
55	*Collyriocincla rufiventris*, Gould,	Buff-bellied Shrike-Thrush,	*a*	...	...	...	...	...	*g*	...	
56	*Falcunculus leucogaster*, Gould,	White-bellied Shrike-Tit,	...	...	...	...	...	...	...	...	
57	*Oreoica cristata*, Lewin.,	Crested Oreoica (Bell-Bird),	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
58	*Rhipidura preissi*, Cab.,	Preiss's Fantail,	*a*	...	...	...	...	...	...	...	
59	*Sauloprocta motacilloides*, V. & H.,	Black Fantail,	...	...	*c*	*d*	*e*	*f*	*g*	...	
60	*Seisura inquieta*, Lath.,	Restless Flycatcher (Grinder),	...	...	*c*	*d*	*e*	*f*	*g*	...	
61	*Micrœca assimilis*, Gould,	Western Flycatcher,	*a*	*b*	...	...	...	...	...	...	
62	*Gerygone culicivora*, Gould,	Western Warbler,	...	...	...	...	...	...	...	...	

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63	*Smicrornis flavescens*, Gould,	Yellow-tinted Smicrornis,	a	b	...	...	...	...	...	...	
64	*Petrœca leggii*, Sharpe,	Scarlet-breasted Robin,	...	...	...	d	e	f	g	h	Seen in all forest country.
65	,, *goodenovii*, V. & H.,	Red-capped Robin,	...	...	...	d	e	f	g	...	
66	*Melanodryas bicolor*, V. & H.,	Hooded Robin,	...	...	...	d	e	f	g	...	
67	*Drymodes brunneopygia*, Gould,	Scrub Robin,	...	..	...	...	e	f	g	...	
68	*Eopsaltria gularis*, Q. et Gaim.,	Grey-breasted Robin (Yellow Robin),	...	...	...	...	...	...	g	...	
69	,, *georgeana*, Q. et Gaim.,	White-bellied Robin,	...	...	...	...	...	...	...	...	
70	*Psophodes nigrogularis*, Gould,	Black-throated Coach-whip-Bird,	...	...	...	...	...	...	...	...	
71	*Malurus splendens*, Q. et Gaim.,	Banded Superb Warbler (Blue-Bird),	...	...	...	...	...	...	g	...	
72	,, *elegans*, Gould,	Graceful Superb Warbler,	...	...	...	...	...	...	...	...	
73	,, *pulcherrimus*,	Blue-breasted Superb Warbler,	...	...	...	...	...	...	...	...	
74	*Amytis macrourus*, Gould,	Large-tailed Wren,	...	...	...	...	...	...	...	...	
75	*Stipiturus malachurus*, Lath.,	Emu Wren,	...	...	...	d	e	f	g	h	
76	*Sphenura longirostris*, Gould,	Long-billed Bristle Bird,	...	...	...	...	...	...	...	...	
77	*Atrichia clamosa*, Gould,	Noisy Scrub Bird,	...	...	...	...	...	...	...	...	
78	*Hylacola pyrrhopygia*, V. & H.,	Cautious Wren,	...	...	...	d	e	f	g	...	
79	*Sericornis maculatus*, Gould,	Spotted Scrub-Tit,	..	...	...	...	e	f	g	...	
80	*Acanthiza apicalis*, Gould,	Western Tit,	...	...	...	...	...	...	...	...	
81	,, *inornata*, Gould,	Plain-coloured Tit,	...	...	...	...	...	...	g	...	
82	*Geobasileus chrysorrhœa*, Q. et Gaim.,	Yellow-rumped Tit,	...	...	c	d	e	f	g	h	
83	*Ephthianura albifrons*, J. & Selb.,	White-fronted Chat,	...	...	...	d	e	f	g	h	
84	*Xerophila leucopsis*, Gould,	White-faced Xerophila,	...	...	c	...	e	f	g	...	

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85	*Pyrrholæmus brunneus*, Gould, .	Red Throat, ...	...	...	...	...	*e*	*f*	*g*	...	
86	*Calamanthus campestris*, . . .	Field Calamanthus, . . .	...	...	...	...	...	*f*	*g*	...	
87	*Anthus australis*, V. & H., . .	Australian Pipit, ...	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
88	*Cincloramphus cantillans*, Gould, .	Black-breasted Skylark, . .	*a*	*b*	...	...	...	*f*	*g*	...	Seen Geographé Bay.
89	,, *rufescens*, V. & H., .	Rufus-tinted Skylark, . . .	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
90	*Sphenæacus gramineus*, Gould, .	Little Grass-Bird, . . .	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
91	*Calamoherpe longirostris*, Gould, .	Long-billed Reed Warbler, . .	*a*	...	...	...	...	...	...	...	
92	*Mirafra horsfieldii*, Gould, . .	Horsfield's Bush Lark, . .	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
93	*Estrilda oculea*, Q. et Gaim., . .	Red-eared Finch, . . .	...	...	...	...	...	...	...	...	
94	,, *castanotis*, Gould, . .	Chestnut-eared Finch, . . .	*a*	*b*	...	*d*	*e*	*f*	*g*	...	
95	*Cinclosoma castanconotum*, Gould, .	Chestnut-backed Ground Thrush,	...	...	...	..	*e*	*f*	*g*	...	
96	*Chlamydodera nuchalis*, J. & Selb.,	Great Bower-Bird, . . .	*a*	*b*	...	...	...	...	...	...	
97	*Corone australis*, Gould, . . .	White-eyed Crow, . . .	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
98	*Pomatostomus superciliosus*, V. & H.,	White-eyebrowed Pomatostomus, .	...	*b*	*c*	*d*	*e*	*f*	*g*	...	
99	*Meliornis longirostris*, Gould, . .	Long-billed Honey-eater, . .	...	...	...	...	...	...	*g*	...	
100	,, *mystacalis*, Gould, . .	Moustached Honey-eater, . .	...	...	...	...	...	...	...	...	
101	*Glyciphila fulvifrons*, Lewin, . .	Fulvous-fronted Honey-eater, .	...	...	...	*d*	*e*	*f*	*g*	*h*	
102	,, *albifrons*, . . .	White-fronted Honey-eater, .	...	...	...	...	*e*	*f*	*g*	...	
103	*Stigmatops ocularis*, Gould. . .	Brown Honey-eater, . . .	*a*	*b*	*c*	*d*	*e*	...	...	...	
104	*Ptilotis vittata*, Cuv., . . .	Singing Honey-eater, . . .	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
105	,, ***cratitia***, Gould, . . .	Wattle-cheeked Honey-eater, .	...	*b*	...	...	...	*f*	*g*	...	
106	,, ***occidentalis***, Cab., . .	Western Honey-eater, . . .	...	...	...	...	...	...	...	...	
107	,, ***leucotis***, Lath., . . .	White-eared Honey-eater, . .	*a*	*b*	...	*d*	*e*	*f*	*g*	...	
108	,, ***ornata***, Gould, . . .	Graceful Honey-eater, . . .	...	...	...	...	...	*f*	*g*	...	

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109	*Ptilotis plumula*, Gould,	Plumed Honey-eater,	...	*b*	...	*d*	...	...	*g*	...	
110	*Certhionyx leucomelas*, Cuv.,	Pied Honey-eater,	...	...	...	...	*e*	*f*	*g*	...	
111	*Acanthogenys rufigularis*, Gould,	Spiny-cheeked Honey-eater,	...	...	...	*d*	*e*	*f*	*g*	...	
112	*Acanthochara carunculata*, Lath.,	Wattled Honey-eater (Wattle Bird),	...	...	...	*d*	*e*	*f*	*g*	...	
113	*Anellobia lunulata*, Gould,	Lunulated Wattle Bird,	...	...	...	...	...	...	...	..	
114	*Acanthorhynchus superciliosus*,	White-eyebrowed Spinebill,	...	...	..	...	...	...	...	...	
115	*Myzomela nigra*, Gould,	Black Honey-eater,	...	*b*	...	*d*	*e*	*f*	*g*	...	
116	*Melithreptus brevirostris*, V. & H.,	Short-billed Honey-eater,	...	...	*c*	*d*	*e*	*f*	*g*	...	Seen in Lower Swan District.
117	,, *gularis*, Gould,	Black-throated Honey-eater,	...	...	*c*	*d*	*e*	*f*	*g*	...	
118	,, *chloropsis*, Gould,	Swan River Honey-eater,	...	...	...	...	...	...	...	...	
119	*Myzantha obscura*, Gould,	Sombre Honey-eater (Miner),	...	...	...	...	...	..	*g*	...	
120	*Dicæum hirundinaceum*, Shaw,	Flower Picker (Mistletoe Bird),	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
121	*Zosterops gouldi*, Bp.,	Green-backed White-eye,	...	...	...	...	...	...	...	...	
122	*Climacteris rufa*, Gould,	Rufus Tree-creeper,	*a*	...	...	...	...	...	*g*	...	
123	*Sittella pileata*, Gould,	Black-capped Sittella,	...	...	...	...	*e*	*f*	*g*	...	
124	*Cuculus pallidus*, Lath.,	Pallid Cuckoo,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
125	,, *flabelliformis*, Lath.,	Fantailed Cuckoo,	?	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
126	,, *insperatus*, Gould,	Brush Cuckoo,	*a*	?	*c*	*d*	*e*	*f*	*g*	...	
127	*Mesocalius palliolatus*, Lath.,	Black-eared Cuckoo,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
128	*Chalcites plagosus*, Lath.,	Bronze Cuckoo,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
129	*Cacatua galerita*, Lath.,	Sulphur-crested Cockatoo,	...	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
130	,, *leadbeateri*, Vigors,	Leadbeater's Cockatoo (Chock-a-lock),	...	...	...	*d*	*e*	*f*	*g*	...	

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131	*Cacatua gymnopis*, Sclater,	Dampier's Cockatoo (Champion Bay Cockatoo),	*a*	*b*	*c*	...	*e*	...	*g*	...	Seen in Champion Bay District.
132	,, *roseicapilla*, Vieill.,	Rose-breasted Cockatoo,	*a*	*b*	...	*d*	*e*	*f*	*g*	...	Do. do.
133	*Licmetis pastinator*, Gould,	Western Long-billed Cockatoo,	...	...	...	...	...	...	...	...	
134	*Calyptorhynchus naso*, Gould,	Western Black Cockatoo,	*a*	...	...	...	...	...	...	...	
135	,, *baudinii*, Vigors,	Baudin's Cockatoo,	...	...	...	...	...	...	...	...	
136	*Polytelis melanura*, Vigors,	Black-tailed Parrakeet, (Whartiu-gale),	...	...	...	?	*e*	*f*	*g*	...	
137	*Platycercus semitorquatus*, Q. et G.,	Yellow-collared Parrakeet ("28" Parrot),	...	...	...	...	...	...	...	...	
138	,, *zonarius*, Shaw,	Banded Parrakeet (Champion Bay "28" Parrot),	...	...	...	...	...	*f*	*g*	...	Obtained Champion Bay District. Probably a local variety.
139	,, *icterotis*, Temm.,	Yellow-cheeked Parrakeet (Rosella),	...	...	...	...	...	...	...	...	
140	,, *spurius*, Kuhl,	Red-capped Parrakeet (King Parrot),	...	...	...	...	...	...	...	...	
141	*Psephotus hæmatogaster*, Gould,	Red-vented Parrakeet,	...	...	...	*d*	*e*	*f*	*g*	...	
142	,, *xanthorrhous*, Gould,	Yellow-vented Parrakeet,	...	...	...	...	*e*	*f*	*g*	...	
143	*Euphema elegans*, Gould,	Elegant Grass Parrakeet,	...	...	...	*d*	*e*	*f*	*g*	...	
144	,, *petrophila*, Gould,	Rock Parrakeet (Rottnest Parrot),	...	...	...	...	...	...	*g*	...	
145	,, *splendida*, Gould,	Splendid Grass Parrakeet,	...	...	...	...	*e*	*f*	*g*	...	
146	*Melopsittacus undulatus*, Shaw,	Warbling Grass Parrakeet,	*a*	*b*	...	*d*	*e*	*f*	*g*	...	

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147	*Calopsittacus novæ-hollandiæ*, Gml.,	Cockatoo Parrakeet,	a	b	...	d	e	f	g	...	
148	*Pezoporus formosus*, Lath.,	Ground or Swamp Parrakeet,	...	...	...	d	e	f	g	h	
149	,, *occidentalis*, Gould,	Western Ground Parrakeet,	...	...	...	...	...	...	g	...	
150	*Trichoglossus porphyrocephalus*, Dietr.,	Porphyrio-crowned Lorrikeet,	...	...	...	...	e	f	g	...	
151	*Phaps chalcoptera*, Lath.,	Bronzewing Pigeon,	a	b	c	d	e	f	g	h	
152	,, *elegans*, Temm.,	Brush Bronzewing Pigeon,	...	b	...	d	e	f	g	h	
153	*Lophophaps ferruginea*, Gould,	Rust-coloured Bronzewing Pigeon,	a	...	...	...	...	...	g	...	
154	*Ochyphaps lophotes*, Temm.,	Crested Bronzewing Pigeon,	a	b	...	d	e	f	g	..	Victoria District (Maitland-Brown).
155	*Geopelia cuneata*, Lath.,	Little Turtle Dove,	a	b	...	d	e	f	g	...	
156	*Leipoa ocellata*, Gould,	Maller Fowl (Gnow),	...	...	...	...	e	f	g	...	
157	*Turnix scintillans*, Gould,	Speckled Turnix-Quail,	a	b	c	...	...	...	...	...	
158	,, *velox*, Gould,	Swift-flying Turnix-Quail,	a	...	...	d	e	f	g	...	
159	*Coturnix pectoralis*, Gould,	Stubble Quail,	...	...	c	d	e	f	g	h	
160	*Synoicus australis*, Lath.,	Swamp Quail,	a	...	c	d	e	f	g	h	
161	,, *sordidus*, Gould,	Sombre Swamp Quail,	...	...	...	...	...	f	g	...	Seen Breaksea Island and Mainland.
162	*Dromaius novæ-hollandiæ*, Lath.,	Emu,	a	b	c	d	e	f	g	...	
163	,, *irroratus*, Bartlett,	Spotted Emu,	a	...	...	...	...	f	g	...	
164	*Eupodotis australis*, Gray,	Australian Bustard,	a	b	c	d	e	f	g	...	
165	*Œdicnemus grallarius*, Lath.,	Southern Stone Plover (Night Curlew),	a	l	c	d	e	f	g	...	
166	*Hæmatopus longirostris*, Vieill.,	White-breasted Oyster-catcher,	a	b	c	d	e	f	g	h	

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167	*Hæmatopus unicolor*, Wagl.,	Sooty Oyster-catcher,	...	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
168	*Sarciophorus pectoralis*, Cuv.,	Black-breasted Plover,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
169	*Charadrius helveticus*, Linn.,	Grey Plover,	...	*b*	...	...	*e*	*f*	*g*	*h*	
170	,, *fulvus*, Gmel.,	Golden Plover,	...	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
171	*Eudromius australis*, Gould,	Australian Dottrel,	...	...	...	*d*	*e*	*f*	*g*	...	
172	*Ægialitis monacha*, Geoff.,	Hooded Dottrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
173	,, *nigrifrons*, Cuv.,	Black-fronted Dottrel,	*a*	...	*c*	*d*	*e*	*f*	*g*	...	
174	,, *ruficapilla*, Temm.,	Red-capped Dottrel,	?	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
175	,, *bicincta*, J. & Selb.,	Double-banded Dottrel,	...	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
176	*Himantopus leucocephalus*, Gould,	White-headed Stilt,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
177	*Cladorhynchus pectoralis*, Dubus,	Banded Stilt (Rottnest Snipe),	...	...	...	...	*e*	*f*	*g*	*h*	
178	*Recurvirostra rubricollis*, Temm.,	Red-necked Avocet,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
179	*Limosa uropygialis*, Gould,	Barred-rumped Godwit,	...	*b*	*c*	*d*	*e*	*f*	...	*h*	
180	*Tringa acuminata*, Horsf.,	Marsh Tringa or Sandpiper,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
181	,, *subarquata*, Gmel.,	Curlew Sandpiper,	?	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
182	,, *albescens*, Temm.,	Little Sandpiper,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
183	,, *crassirostris*, Temm. & Schleg.,	Great Sandpiper,	...	*b*	*c*	*d*	*e*	...	...	...	
184	*Strepsilas interpres*, Linn.,	Turnstone,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
185	*Actitis hypoleucos*, Linn.,	Common Sandpiper,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
186	*Gallinago australis*, Lath.,	New Holland Snipe,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
187	*Rhynchæa australis*, Gould,	Australian Rhynchæa (Painted Snipe),	...	*b*	*c*	*d*	*e*	*f*	*g*	..	
188	*Numenius cyanopus*, Vieill,	Curlew,	?	*b*	*c*	*d*	*e*	*f*	*g*	*h*	

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189	*Numenius uropygialis*, Gould,	Wimbrel,	?	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
190	,, *minor*, Schleg.,	Little Wimbrel,	?	*b*	*c*	*d*	*e*	*f*	*g*	...	
191	*Geronticus spinicollis*, Jameson,	Straw-necked Ibis,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	Victoria and Swan Districts (Cowan).
192	*Grus australasianus*, Gould,	Australian Crane (Native Companion),	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	More than once noticed to the west of Eucla, Gascoyne R., 1888 (Lefroy).
193	*Xenorhynchus asiaticus*, Lath.,	Black and White Stork (Jaberoo),	*a*	*b*	*c*	*d*	*e*	...	...	...	
194	*Ardea pacifica*, Lath.,	Pacific Heron,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
195	,, *novæ-hollandiæ*, Lath.,	White-fronted Heron (Blue Crane),	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
196	*Herodias melanopus*, Wagl.,	Spotless Egret,	*a*	*b*	*c*	*d*	*e*	*f*	...	...	
197	,, *alba*, Linn.,	Egret or White Crane,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
198	*Demiegretta sacra*, Gmel.,	Blue Reef Heron (and the White),	*a*	*b*	*c*	*d*	?	*f*	*g*	*h*	
199	*Nycticorax caledonicus*, Lath.,	Nankeen Night Heron,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
200	*Botaurus poicilopterus*,	Australian Bittern,	...	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
201	*Butoroides flavicollis*, Lath.,	Yellow-necked Mangrove Bittern,	*a*	*b*	*c*	*d*	*e*	...	*g*	...	
202	*Porphyrio bellus*, Gould,	Azure-breasted Porphyrio-Coot,	*a*	...	...	...	...	...	...	...	
203	*Tribonyx ventralis*, Gould,	Black-tailed Tribonyx,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	...	
204	*Fulica australis*, Gould,	Australian Coot,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
205	*Hypotænidia philippensis*, Linn.,	Pectoral Rail (Land-rail),	?	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
206	,, *brachipus*, Swains,	Lewin's Water-Rail,	...	...	...	*d*	*e*	*f*	*g*	*h*	

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207	*Porzana palustris*, Gould,	Little Water-Crake,	...	...	...	*d*	*e*	*f*	*g*	*h*	
208	,, *tabuensis*, Gmel.,	Tabuan Water-Crake,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
209	*Cygnus atratus*, Lath.,	Black Swan,	*a*	...	*c*	*d*	*e*	*f*	*g*	*h*	
210	*Cereopsis novæ-hollandiæ*, Lath.,	Cereopsis Goose (Cape Barren),	...	...	...	...	...	*f*	*g*	*h*	Bald Island (N. W. M'Kail).
211	*Branta jubata*, Lath.,	Maned Goose or Wood Duck,	*a*	...	*c*	*d*	*e*	*f*	*g*	*h*	
212	*Casarca tadornoides*, Jard.,	Chestnut-breasted Shieldrake (Mountain Duck),	...	...	...	...	*e*	*f*	*g*	*h*	
213	*Anas superciliosa*, Gmel.,	Australian Wild Duck (Black Duck),	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
214	,, *castanea*, Eyton,	Australian Teal,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
215	*Stictonetta nævosa*, Gould,	Freckled Duck,	...	...	...	...	*e*	*f*	*g*	*h*	
216	*Spartula rhynchotis*, Lath.,	Australian Shoveller,	...	...	...	*d*	*e*	*f*	*g*	*h*	
217	*Malacorhynchus membranaceus*, Lath.,	Pink-eared Duck (Whistling Duck),	*a*	!	*c*	*d*	*e*	*f*	*g*	*h*	
218	*Dendrocygna eytoni*, Gould,	Eyton's Tree Duck,	...	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
219	*Nyroca australis*, Gould,	White-eyed Duck (Widgeon),	...	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
220	*Erismatura australis*, Gould,	Blue-billed Duck,	...	...	...	...	...	*f*	*g*	...	
221	*Biziura lobata*, Shaw,	Musk Duck,	...	...	...	*d*	*e*	*f*	*g*	*h*	
222	*Larus pacificus*, Lath.,	Pacific Gull,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
223	,, *novæ-hollandiæ*, Steph.,	Silver Gull,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	Seen in King George's Sound.
224	,, *longirostris*, Masters,	Long-billed Gull,	...	...	...	...	...	...	...	...	
225	*Stercorarius antarcticus*, Less.,	Great Skua,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	

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226	*Sterna caspia*, Pall.,	Caspian Tern (Diver),	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
227	,, *bergii*, Licht.,	Torres' Straits Tern,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
228	,, *dougalli*, Mont.,	Graceful Tern,	*a*	...	*c*	*d*	...	...	...	...	
229	,, *fuliginosa*, Gmel.,	Sooty Tern,	*a*	*b*	*c*	*d*	*e*	...	*g*	...	
230	,, *anæstheta*, Scop.,	Panayan Tern,	...	*b*	*c*	*d*	*e*	...	?	...	
231	,, *nigrifrons*, Masters,	... ...	...	...	*c*	*d*	...	...	...	...	
232	*Sternula nereis*, Gould,	... ...	...	...	...	*d*	*e*	*f*	*g*	*h*	
233	*Hydrocheledon hybrida*, Pall.,	Marsh Tern,	...	*b*	*c*	*d*	*e*	*f*	*g*	...	
234	*Anous stolidus*, Linn.,	Noddy Tern (Guano Bird),	*a*	*b*	*c*	*d*	*e*	...	...	...	
235	,, *tenuirostris*, Temm.,	Lesser Noddy,	...	...	*c*	...	*e*	...	...	...	
236	*Diomedea exulans*, Linn.,	Wandering Albatross,	...	...	...	...	*e*	*f*	*g*	*h*	
237	,, *cauta*, Gould,	Sly Albatross,	...	...	...	*d*	*e*	*f*	*g*	*h*	
238	,, *culminata*, Gould,	Culminated Albatross,	...	...	...	...	*e*	*f*	*g*	*h*	
239	,, *chlororhyncha*, Gmel.,	Yellow-nosed Albatross,	...	...	...	...	*e*	*f*	*g*	*h*	
240	,, *melanophrys*, Temm.,	Black-eyebrowed Albatross,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
241	,, *fuliginosa*, Gmel.,	Sooty Albatross,	...	...	...	...	*e*	*f*	*g*	*h*	
242	*Fulmarus giganteus*, Gmel.,	Giant Petrel,	...	...	...	...	*e*	*f*	*g*	*h*	
243	,, *gelidus*, Gmel.,	Great Grey Petrel,	...	...	...	...	*e*	*f*	*g*	*h*	
244	,, *glacialoides*, Smith,	Silver-grey Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
245	*Pterodroma solandri*, Gould,	Solander's Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
246	,, *lessoni*, Garn.,	White-headed Petrel,	...	...	*c*	...	*e*	*f*	*g*	*h*	
247	,, *leucoptera*, Gould,	White-winged Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
248	,, *cærula*, Gmel.,	Blue Petrel,	...	...	...	...	*e*	*f*	*g*	*h*	

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249	*Puffinus nugax* (?), Sol.,	Allied Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	Obtained Houtman's Abrolhos.
250	,, *brevicaudus*, Brandt.,	Short-tailed Petrel } Mutton Birds,	...	...	...	*d*	*e*	*f*	*g*	*h*	
251	,, *carneipes*, Gould,	Fleshy-footed Petrel } Mutton Birds,	...	...	...	...	...	...	..	...	
252	,, *sphenurus*, Gould,	Wedge-tailed Petrel } Mutton Birds,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
253	*Daption capensis*, Linn.,	Cape Petrel,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
254	*Prion turtur*, Smith,	Dove-like Blue Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
255	,, *banksii*, Smith,	Banks' Blue Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
256	,, *vittata*, Forst.,	Broad-billed Blue Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
257	*Procellaria nereis*, Gould,	Grey-backed Storm Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
258	,, *oceanica*, Banks,	Yellow-webbed Storm Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
259	,, *melanogaster*, Gould,	Black-bellied Storm Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
260	,, *grallaria*, Vieill,	White-bellied Storm Petrel,	...	...	...	*d*	*e*	*f*	*g*	*h*	
261	,, *fregata*, Linn.,	White-faced Storm Petrel,	...	...	...	...	...	*f*	*g*	*h*	
262	*Pelecanus conspicillatus*, Temm.,	Australian Pelican,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	
263	*Graculus novæ-hollandiæ*, Steph.,	Australian Cormorant,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
264	,, *varius*, Gmel.,	Pied Cormorant,	...	...	...	*d*	*e*	*f*	*g*	...	
265	,, *leucogaster*, Gould,	White-breasted Cormorant,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
266	,, *melanoleneus*, Vieill,	Little Cormorant,	*a*	...	*c*	*d*	*e*	*f*	*g*	*h*	
267	,, *sticlocephalus*, Bp.,	Little Black Cormorant,	*a*	...	*c*	*d*	*e*	*f*	*g*	*h*	
268	*Phæton candidus*, Briss.,	White-tailed Tropic Bird,	...	...	*c*	*d*	...	...	...	...	Houtman's Abrolhos (G. K. Beddoes, C.E.).

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269	*Phaeton rubricauda*, Bodd.	Red-tailed Tropic Bird,	*a*	*b*	*c*	...	*e*	...	...	...	Seen Houtman's Abrolhos.
270	*Sula serrator*, Banks,	Australian Gannet,	...	...	*c*	*d*	*e*	*f*	*g*	*h*	
271	*Podiceps cristatus*, Linn.,	Tippet Grebe,	...	...	...	*d*	*e*	*f*	*g*	*h*	
272	,, *nestor*, Gould,	Hoary-headed Grebe,	*a*	...	...	*d*	*e*	*f*	*g*	*h*	
273	*Catarractes chrysocome*, Lath.,	Crested Penguin,	...	*b*	...	...	*e*	*f*	*g*	*h*	Near Hamelin Harbour (Tomb).
274	*Eudyptula minor*, Gould,	Little Penguin,	...	...	...	*d*	*e*	*f*	*g*	*h*	
	Circus gouldii, Bp.,	Allied Harrier (Swamp Hawk),	*a*	...	*c*	*d*	*e*	*f*	*g*	*h*	
	Smicrornis brevirostris, Gould,	Short-billed Smicrornis,	...	...	...	*d*	*e*	*f*	*g*	...	
	Estrilda ruficauda, Gould,	Red-tailed Finch,	*a*	*b*	*c*	*d*	*e*	...	...	...	
	Calamanthus fuliginosus, V. & H.,	Striated Calamanthus,	...	...	...	...	...	*f*	*g*	*h*	
	Corvus coronoides, V. & H.,	Hazel-eyed Crow,	*a*	*b*	*c*	*d*	*e*	*f*	*g*	?	
	Trichoglossus versicolor, Vigors,	Varied Lorrikeet,	*a*	*b*	*c*	...	...	...	...	...	
	Malurus leucopterus, Q. et Gaim.,	White-winged Superb Warbler,	...	...	...	...	*e*	*f*	*g*	...	
	,, *callamies*, Gould,	Turquoisine Superb Warbler,	...	...	...	...	*e*	...	*g*	...	
	Sternula inconspicua, Masters,	... ...	...	...	*c*	...	...	...	..	...	

AUSTRALIA
English Miles
NEW GUINEA
CORAL SEA
PACIFIC OCEAN
INDIAN OCEAN
Arafura Sea
Gulf of Carpentaria
Torres Strait
Great Australian Bight
Bass Strait
WESTERN AUSTRALIA
Northern Territory of South Australia
NORTH QUEENSLAND
SOUTH QUEENSLAND
SOUTH AUSTRALIA
NEW SOUTH WALES
VICTORIA
TASMANIA
New Caledonia
Norfolk I.
Lord Howe I.
Timor
Flores
Sumbawa
Lombok
Sandalwood or Sumba
Madura
Kimberley District
Great Sandy Desert
The Great Victoria Desert
Tropic of Capricorn
Perth
Fremantle
Albany
Geraldton
Broome
Palmerston
Burketown
Cooktown
Rockhampton
Maryborough
Brisbane
Port Macquarie
Newcastle & Port Hunter
Sydney & Port Jackson
Goulburn
Albury
Melbourne
Ballarat
Sandhurst
Geelong
Portland
Adelaide
Port Augusta
Kangaroo I.
Eyre's Pen.
Hobart
Launceston
C. York
Cape York Penin.
Arnhem Ld.
John Bartholomew & Co. Edin.

Pharmacology of Morphine and its Derivatives. By **D. B. Dott**, F.C.S., F.I.C., and **Ralph Stockman**, M.D., *Research Scholar of the British Medical Association.*

(Read July 7, 1890.)

It is necessary for a clear understanding of the chemical changes which occur on treating morphine in various ways, and for comparing its physiological action with that of the bodies so formed, that we should summarise at some length the exact position of our present knowledge regarding its chemistry and pharmacology.

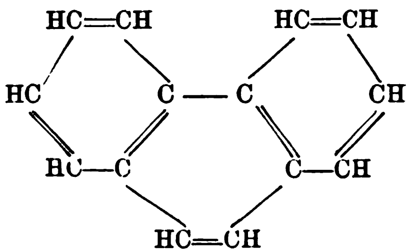
Morphine has the composition represented by the formula $C_{17}H_{19}NO_3$. It is generally supposed to crystallise with one molecule of water, but there is reason to believe that the composition of the hydrated base is really 8 $C_{17}H_{19}NO_3$, 9 H_2O, the water being present in the proportion of $1\frac{1}{8}$ molecule. Morphine is a monacid base, forming neutral and readily soluble salts with all the stronger acids. Like all alkaloids, it is built up on the ammonia (NH_3) type, and is a tertiary amine, all the hydrogen atoms having been replaced by more or less complex radicals. This is shown to be the case by the fact that when treated with methyl iodide, the latter body becomes directly united to the morphine molecule, combining with the nitrogen and so forming a quaternary ammonium base, analogous to tetramethyl-ammonium iodide. Such alkaloidal derivatives are therefore to be regarded as *addition* compounds, as indicated in the formula $C_{17}H_{19}NO_3$, CH_3I. To How belongs the credit of having been the first to form these compounds, by acting on morphine, codeine, strychnine, and other alkaloids with alkyl-iodides. He fell into the error, however, of regarding them as substitution compounds, in which hydrogen had been replaced by the different alcohol radicals.

Although the constitution of morphine is not exactly known, much information has been accumulated regarding it, and our knowledge of certain of the groups contained in the molecule is considerable, and of practical importance. The first distinct advance in such knowledge was made by Beckett and Wright, who found that by treating morphine with acetic acid and acetic anhydride, first one and then another hydrogen atom were replaced by the

acid radical, such derivatives being *substitution* compounds. The substitution could not by any means be carried further. That is to say, monoacetyl- and diacetyl-morphine were formed, but not the triacetyl or any higher derivative. Butyric and benzoic anhydrides were tried with precisely similar results. Whence it is manifest that the morphine molecule contains two hydroxyl groups, a fact which has been confirmed by the experiments of Hesse and of Gerichten. On referring to the original paper of Beckett and Wright, it will be observed that these chemists adopt the double formula $C_{34}H_{38}N_2O_6$ for the molecule of morphine, so that they use the terms diacetyl- and tetracetyl-morphine for what we have called monoacetyl- and diacetyl-morphine. In a former communication to this Society we have fully discussed the objections to the higher formula. It seems to be clearly established that the third atom of oxygen is either connecting or ketone oxygen, *i.e.* oxygen which acts as the connecting link between two atoms of carbon, or otherwise has its combining powers saturated by union with one atom of carbon. The next important point to be noticed is that those two hydroxyl groups in the morphine molecule have different values—fulfil different functions. It had long been known that morphine is readily soluble in solutions of the caustic alkalies and in lime water, but it was reserved for Chastaing to show that this ready solubility is due to the formation of definite compounds with the alkalies. These bodies have the general composition M, $C_{17}H_{18}NO_3$, H_2O or MOH,$C_{17}H_{19}NO_3$, where M represents a monatomic metal such as sodium. That is to say, that while the morphine molecule contains two hydroxyl groups, only one of these is capable of having its hydrogen replaced by a metal. In this respect and in some others, such as its coloration with ferric salts, morphine resembles a phenol. Indeed the readiness with which morphine reacts with potash, evolving heat and forming a crystalline compound, is very suggestive of phenol (carbolic acid). In the same way that phenol (C_6H_5OH) forms ethers, as methyl-phenylether ($C_6H_5O.CH_3$) so morphine ($C_{17}H_{18}NO_2OH$) gives us ethers, the morphine-methyl ether being codeine ($C_{17}H_{18}NO_2.OCH_3$) which is found along with morphine in opium. By a study of the products of decomposition of codeine under different conditions, additional light is thrown on the constitution of morphine. Anderson first showed that codeine is

morphine in which one hydrogen atom has been replaced by methyl, and Grimaux in 1881 succeeded in forming codeine from morphine. Beckett and Wright, by acting on codeine with acetic anhydride, found that they could only introduce one acetyl group, the other hydroxyl hydrogen (of the morphine molecule) having been already replaced by methyl. For the same reason, Gerichten found that by submitting codeine to the action of phosphorus pentachloride, he was able only to replace one hydroxyl by chlorine, the compound obtained having the formula $C_{18}H_{20}ClNO_2$ (chlorocodide). By continued action of the pentachloride, the compound $C_{18}H_{19}Cl_2NO_2$ was obtained, whence it is evident that the third atom of oxygen in the morphine molecule does not exist as hydroxyl.

By oxidation of morphine Chastaing obtained picric acid, proving that morphine contains the benzene chain. Wright and Mayer obtained pyridine on distilling morphine with potash. By distillation with zinc dust Gerichten and Schrötter obtained pyridine, phenanthrene, pyrrol, trimethyl-amine, and other compounds. Although up to the present time the constitution of morphine has not been certainly determined, there is reason to believe that it is essentially a phenanthrene derivative, containing alkyl and hydroxyl groups. Phenanthrene consists, as it were, of three benzene rings welded together, and is represented graphically as follows :—



The nitrogen probably replaces one of the CH groups as in pyridine, and it would appear from the number of hydrogen atoms in the morphine molecule that the latter must be a hydro derivative, as piperidine is of pyridine. We are aware that it has been recently suggested that there are a methyl and an ethyl radical directly combined with the nitrogen, in which case the nitrogen could not be combined as it is in pyridine and chinoline. In view of the fact that pyridine has been obtained from morphine, and from the

analogy that several of the other alkaloids are known to be pyridine or chinoline derivatives, one is inclined to regard morphine as of the same class until conclusive proof is advanced to the contrary.

Physiological Action of Morphine.

There is now complete agreement among authors regarding the general symptoms observed after the administration of morphine, although many points of practical, as well as theoretical, interest still require elucidation. All recent observers state that the central nervous system is the part primarily affected, and have described the symptoms as divisible into two stages—a narcotic and a tetanic.

Frogs.—After 2–5 centigrams of a soluble salt of morphine have been given subcutaneously to a frog, the animal becomes in a few minutes dull and heavy, and shows a distinct inclination to remain quietly in one position. When pinched or otherwise irritated, it at first jumps quite well, but very shortly loses its power of accurate muscular co-ordination. It then ceases to move away even when pinched, and if placed on its back remains in that position. The corneal reflex becomes abolished, while the spinal reflexes are diminished, but seldom wholly disappear. All these symptoms indicate depression of the brain and spinal cord, and Witkowski has pointed out that the cerebral lobes succumb first, the other parts of the central nervous system becoming involved in order from before backwards.

The frog remains in this helpless narcotised condition for a varying time until the second stage supervenes, the spinal reflexes gradually becoming more and more exaggerated until tetanic spasms occur spontaneously and on stimulation. After a longer or shorter period exhaustion ensues, the frog then responding to stimulation by a mere twitch. It may finally die or gradually recover.

During both stages the brain is deeply narcotised, as irritation of the cornea during the tetanic condition brings on a general spasm but no closure of the eyelid. The spasms are spinal, as section of the cord at the atlanto-occipital membrane does not stop them; the succeeding exhaustion also arises from the spinal cord, as the motor nerves and muscles are found to be only slightly diminished in electric irritability. A very distinct peculiarity of the morphine

tetanus is that after each spasm a period of exhaustion ensues, and there is no further tetanus—either spontaneous or reflex—until a short interval has elapsed. Not only, therefore, is the spinal cord abnormally easily excited, but it is abnormally easily exhausted. A healthy frog will react to stimulation time after time in an equal degree and gently, while the morphinised frog gives out all its energy for the time being in one prolonged and exaggerated spasm. According to Witkowski, the apparent stimulation is in reality a kind of paralysis, and is simply a failure on the part of the spinal cord to husband and properly distribute its resources, this disarrangement resulting in an unmanageable and violent explosion of nerve energy. Hence it requires a rest until more is stored up. We have observed, however, that the smaller the dose by which tetanus is induced, the less marked is the tendency to exhaustion, and if the animal survive a few days this tendency to exhaustion after each spasm wears off, and the tetanic attacks cannot be distinguished from those of strychnine.

Smaller doses cause only narcosis and depression, followed, generally on next day or earlier, by a marked increase in the reflex excitability, not amounting to tetanus. Very small doses simply cause slight narcosis.

That small doses of morphine depress the reflex and conducting powers of the spinal cord can be shown without much difficulty. Meihuizen, using decapitated frogs (Türck's method), found that there was depression, followed by increase of reflexes. This has been confirmed by Witkowski and by ourselves in many experiments. To confirm these results obtained by Türck's method, we administered morphine to decapitated frogs, and found that, although in many cases the spinal reflexes were very slightly depressed, yet in others the depression was extremely marked, and was succeeded in due time by tetanus. As in these cases the brain was removed, the primary depression and subsequent tetanus must have been quite independent of it, and due solely to an action on the cord. Moreover, *small* doses of morphine directly injected into the aorta of decapitated frogs (so as to make the results independent of irregularities in absorption from the stomach or subcutaneous tissue) gave similar results. Thus—

Expt. 1.—Frog pithed; right aorta tied.

11.21.—0·01 grm. morphine hydrochlorate in 1 cub. cent. water into left aorta.

There gradually ensued indifference to mechanical stimuli. At first a slight pinch with forceps was felt, later it was not felt, and had gradually to be increased in severity to elicit a reaction.

11.38.—Requires a very severe pinch to elicit a reaction.

11.50.—Reflexes slightly exaggerated.

12.0.—Reflexes much more exaggerated. On stimulation gives a tetanic kick out.

12.20.—Still slight tetanus on stimulation, but is rapidly becoming exhausted.

12.30.—Is now much exhausted; only gives a slight spasmodic twitch on stimulation.

In such experiments the cutting off of the blood supply to the cord causes much more rapid exhaustion than would otherwise ensue. Similar results were got with doses of 5 milligrammes.

In the descriptions of the morphine action hitherto published it has always been assumed that the occurrence and sequence of the two stages of narcotism and tetanus are a necessary result of the action of the drug on the nerve cells, the explanation given being that morphine "first depresses" and "then stimulates" the spinal cord. Witkowski advances the opinion that both stages are really the result of paralysis. We have found, however, that the sequence of depression and tetanus is entirely a question of dosage, and of how much morphine reaches the spinal cord. As we have seen, an ordinary dose of the alkaloid first depresses the cord, and this is followed by tetanus. When a *minimum narcotic dose* is given the narcosis is not deep, but no tetanic stage ensues on the narcotic, and hence it is evidently necessary to give a dose of a certain size if we wish to get both stages. When such a dose is given, what happens is that the morphine is but slowly absorbed from the stomach or subcutaneous tissue, only a small portion reaches the spinal cord at first, and hence the depression; but as more morphine becomes absorbed, more of it comes in contact with nerve cells of the cord, and as a result we get tetanus.

Tetanus, we have found, can be induced at once without any preliminary depression if the morphine be thrown into the circulation so as to immediately reach the cord in sufficient quantity.

Hence it is by no means a necessary consequence of the action of morphine that depression succeeded by tetanus should occur. The following experiment shows this decisively.

Expt. 2.—The brain of a frog was destroyed. Next day it had recovered from the pithing. A ligature was passed round the lumbar region, excluding the lumbar nerves; the right aorta was tied; a cannula was inserted into the left aorta.

11.40.—0·035 grms. morphine hydrochlorate in 1 cub. cent. water was injected into the cannula. At once there was rigid tetanus.

11.42.—Another tetanic spasm.

11.44.—Having frequent spontaneous rigid spasms.

12.30.—The spasms have continued, but are gradually getting feebler.

12.45.—Quite exhausted. No response on stimulation.

It is necessary to tie the vessels in the lumbar region to ensure that sufficient of the morphine solution reaches the cord at once, and also (although this is not of such importance) to protect the motor nerves from the depressing influence of the morphine. If this precaution be neglected the experiments never succeed so well, as the greater part of the morphine, instead of going to the spinal cord, gets distributed over the whole body. Tetanus generally appears in such cases, but rather late, although never nearly so late as when the morphine is given subcutaneously.

In the present state of our knowledge it is useless even to speculate as to what changes morphine brings about in the nerve substance of the cord. Small amounts of the alkaloid coming in contact with the grey nerve cells interfere with their vitality and chemical changes only in so far as to inhibit or depress their proper activity, whereas larger amounts cause much more profound changes, apparently leading to irregular and violent chemical action, which leads to similar irregular and violent discharges of nerve energy, followed by extreme exhaustion. During the first condition the cells are more or less incapable of conducting impulses or acting reflexly, whereas in the second they are completely changed in these respects, and respond vigorously to the slightest impression. It is impossible to characterise the changes more definitely.

The delay in the tetanus when morphine is given subcutaneously or by the stomach can be accounted for, however, in two ways.

We have seen that a certain amount of morphine is necessary to cause the tetanus, and therefore (1) we may suppose that the nerve cells have a special affinity for the morphine, that they abstract it from the blood and store it up, and that it is only when they have absorbed and retained sufficient to bring about the requisite changes that tetanus comes on; or (2) we may suppose that the morphine is not stored up in the nerve cells, but that, passing frequently through them dissolved in the blood, it gradually brings about changes which result in tetanus.

With regard to the action of morphine on motor nerves great differences of opinion have been expressed by different experimenters. The older observers, finding that the motor nerves were considerably depressed in electric irritability at the end of the poisoning, concluded that the alkaloid had a directly depressing effect on them. Witkowski, however, is of opinion that this is due to exhaustion after the prolonged tetanus, for on dividing one sciatic nerve, and then administering a large dose of morphine, he found that the divided nerve had remained perfectly excitable, while the undivided nerve was more or less depressed. Vulpian also found that in rabbits the motor nerves remain unaffected.

We have repeated Witkowski's experiment frequently, with a result similar to his, but we are of opinion that his conclusion is erroneous. The result must be due to the morphine not reaching the terminations of the motor nerves in sufficient quantity to paralyse them markedly, for we found when we injected a solution into the aorta, so as to make sure of it reaching the terminations of the motor nerves, that these were paralysed. The amount required is, however, large, and the end-organs of motor nerves are certainly not very sensitive to the action of morphine. The following is one of the experiments.

Expt. 3.—Frog pithed. Sciatic nerves excitable well at 200 mm. Du Bois Reymond Coil.

12.35.—0·06 grm. morphine hydrochlorate in 1¾ cub. cent. water injected into abdominal aorta just above bifurcation.

At once there was marked diminution in the excitability of sciatic nerves.

12.45.—Sciatic nerves excitable only to strongest currents. The

muscles are also diminished somewhat in irritability, but not to anything like the same extent as the nerves.

1.5.—Sciatics not excitable at 0. Muscles contract fairly well to moderate current.

In another frog, when 0·0425 grm. in 1½ cub. cent. water was injected into the abdominal aorta, the sciatics were inexcitable to strongest current in 11 minutes, while the muscles of the legs were still quite excitable to direct stimulation. Small doses, such as 0·01 grm., given in same way had a scarcely appreciable effect on the motor nerves.

In another frog the following experiment was performed as confirmation.

Expt. 4.—Frog pithed; left femoral artery tied; sciatic nerves excitable at 220 mm.

2.28.—0·05 grm. morphine hydrochlorate subcutaneously in solution.

3.40.—Both nerves excitable at 220 mm.

The frog passed through the narcotic and tetanic stages. Next day completely exhausted. Left (protected) sciatic nerve stimulated at 100 mm. gives good contraction of leg muscles; right (unprotected) sciatic at 0 mm. gives scarcely perceptible twitch of muscles.

In this experiment both sciatic nerves were subjected to the exhaustion consequent on the tetanus; but the right nerve, in addition, was exposed to the action of the morphine circulating in the blood. The protected nerve diminished very much in excitability (in consequence of the tetanus plus the cutting off of its blood supply), but the unprotected nerve wholly lost its excitability, and this difference can only be attributed to the action of the morphine on the latter.

On repeating Vulpian's experiment on rabbits, we got (contrary to his results) paralysis of the motor nerves. Thus—

Expt. 5.—Rabbit was put under ether. At 12.5 the right femoral artery was tied and the right sciatic nerve divided. A cannula was inserted into the left femoral artery, and the left sciatic nerve divided. Both nerves excitable at 90 mm.

12.10.—0·2 grm. morphine hydrochlorate was injected into the left femoral artery.

12.20.—R. sciatic nerve quite excitable at 90 mm. L. sciatic at 70 mm. gives very feeble twitch of muscles.

12.30.—R. sciatic nerve gives feeble twitch of muscles at 90 mm., fair contraction at 70 mm. L. sciatic nerve gives no response at 0. Muscles are equally excitable on both sides to direct stimulation.

In another experiment of the same kind 0·1 grm. abolished the excitability of the sciatic nerve in eighteen minutes. It is evident therefore that very large doses of morphine do paralyse the terminations of motor nerves, but that ordinarily death ensues before the depression is very marked. With small therapeutic doses it is hardly possible that even a trace of this action is present.

On sensory nerves and their terminations morphine, even in large doses, has probably a scarcely appreciable effect. The condition of affairs is no doubt much the same as with motor nerves,—that is, if large doses could be applied directly to the nerve terminations, the morphine would act as a paralysant to the nerve tissue, but by the ordinary methods of administration its effect is practically *nil*. It is evident during the tetanic stage that the sensory nerves are perfectly acute, as the slightest stimulation of the skin brings on a spasm. The common method of applying opium fomentations as an anodyne has probably no action except what results from heat and moisture, as the alkaloids cannot penetrate the skin, and even if they did do so, would not be in sufficient amount to paralyse the sensory nerve terminations.

Mammalia.—The higher animals are affected by morphine in the same way as frogs, but there are certain differences in the symptoms, depending not on a different action of the poison, but on differences in the nervous system, and on the closer interdependence of its various parts in the higher animals. Frogs live long after the respiratory centre is paralysed, whereas mammals die at once, so soon as this centre is thrown out of action, hence the longer continuance of the symptoms and the marked development of the tetanic stage in the former. The greater importance of the spinal cord relatively to the brain in batrachians has no doubt also an effect in modifying the symptoms. Rabbits, cats, and dogs pass through a narcotic and a tetanic stage if the dose be large enough. With small doses only the first stage is seen. (For doses and particulars, see Tables of Experiments.)

The condition of affairs is the same as with frogs. Small doses depress the brain and spinal cord, while larger ones throw the cord into a condition of hyper-excitability. Whether large doses affect

the cerebrum in the same way remains doubtful, as death occurs early in such cases from paralysis of the respiratory centre. The only method of determining this point would be to apply morphine directly to the brain. We were unable to carry out such experiments, but the results of certain observations by Deguise, Dupuy, and Leuret seem to show that large doses of morphine applied to the brain directly do produce convulsions. In Part II. of their paper, Experiments 11, 12, 13, 16 and others (dogs and rabbits), the injection of morphine solutions into the lateral ventricle, or their application to the cortex, produced well-marked convulsions of an epileptiform character.

For purposes of comparison it is necessary to refer briefly to the action of morphine on some of the other systems. In dogs and cats, nausea, vomiting, and slight diarrhœa are always very prominent symptoms, unless the dose be very small. In man, as is well known, small doses cause constipation, but nausea and vomiting, with sometimes slight looseness of the bowels, are not infrequently observed. The exact reason of this is still to be explained, but Alt has recently shown that morphine given hypodermically is very rapidly secreted into the stomach, and no doubt the consequent irritation is sufficient to account for the intestinal symptoms.

The heart and blood-vessels are affected very slightly, and only secondarily to the nervous system. Owing to dulling of the vasomotor centre there is slight loss of tone in the arteries, accompanied by a correspondingly slight fall of blood pressure. The pupil is contracted during the narcotic stage, but as soon as tetanic symptoms develop, it begins to dilate, and often is fully dilated during the greater part of the poisoning. The respiratory centre is very much depressed, and respiration greatly slowed, even with small doses.

As the object of our research was to compare the action of morphine with the actions of a number of bodies closely related to it chemically, we thought that the best plan to obtain an accurate idea of their relative toxicity and effects would be to ascertain the minimum dose which produced visible symptoms, and by gradually increasing it until the minimum lethal dose was reached, we had a complete picture of the general action of the substance. Owing to the large number of alkaloids investigated by us, it was necessary to confine our experiments almost entirely to frogs and rabbits, the information thus obtained being sufficient for purposes of com-

parison. In a few instances, where an emetic action was suspected, the substances were administered to dogs also. In our experiments we used morphine hydrate dissolved in acetic acid and water, or morphine hydrochlorate, obtained from J. F. Macfarlan & Co., Edinburgh.

Toxicity of Morphine.

There are wide discrepancies in the statements regarding the minimum lethal dose of morphine in frogs, for, while Fröhlich fixes it at 0·01 grm., Witkowski states that he has seen frogs recover after 0·05 grm. hydrochlorate. We found that it varied a good deal according to the condition of the frog and the season of the year, but that the ordinary lethal dose for a medium-sized frog is from 3–5 centigrms. (*R. temporaria*). The frog, after passing through the narcotic stage, remains tetanised or completely exhausted for several days, and ultimately dies of exhaustion. One to two centigrms. cause a marked narcotic stage, followed next day by an increase of reflexes, not amounting to tetanus. Five milligrms. caused very slight narcosis, which was not followed by any visible increase of reflexes.

In rabbits also we could get no information regarding the minimum physiological dose, while the minimum lethal dose as given by different investigators (see Table) varied so much, that to

Table showing the Minimum Lethal Dose of Morphine in Rabbits, as fixed by previous Experimenters.

Name.	Weight of Rabbit in Grms.	Salt of Morphine used.	Dose in Grms.	Dose reckoned as Morphine Hydrate.	Dose per Kilo. Body Weight.	Remarks.
British Medical Association Commission	1360	Hydrochlorate	0·77	0·627	0·461	Death with severe convulsions in about one hour.
Do. do. . . .	1360	Meconate	0·65	0·456	0·335	Death with convulsions in 1 to 2 hours.
Fröhlich . . .		Hydrochlorate	0·3	0·242		2 Experiments only. Death in 10 and 24 hours respectively.
Koning* . . .	330–570	Hydrochlorate	0·47	0·379	0·66 1·15–	
Camus . . .		Hydrochlorate	1·0	0·806		Death in 4¼ hours.
Falck	1250–1500	Sulphate	1 to 2	0·799–1·6	0·63–1·08	Death in from 1 hour 36 minutes to 3 hours 27 minutes.
Do.	1310	Hydrochlorate	1·0	0·806	0·61	Death in 2 hours 45 minutes.

* Quoted by Fröhlich.

determine both points we had to institute special experiments. The results of these are given in the following Tables.

Three decigrms. of morphine hydrate given subcutaneously was usually a fatal dose for medium-sized rabbits. This is equal to about 0·37 grm. of morphine hydrochlorate, sulphate, or tartrate, to 0·4 grm. acetate, and 0·43 grm. meconate. Certain rabbits were killed with less, and others required more. The minimum lethal dose of any substance can hardly be fixed with complete accuracy, as the rate of absorption and excretion, the condition of health of the animal, along with other less evident factors, all influence the result and make it variable. In addition to our own results, Lenhartz has found the minimum lethal dose of morphine per kilo. in dogs to vary from 0·27 to 0·59 grm., while recovery took

Table of Experiments made to fix the Minimum Lethal Dose of Morphine Hydrate in Rabbits.

No.	Weight of Rabbit in Grms.	Dose of Morphine Hydrate in Grms.	Dose per Kilo. of Body Weight.	Deeply Narcotised in	Pupils after Contracting began to Dilate in	Increased Reflex apparent in	First Marked Tetanic Spasm in	Result.
				min.	min.	hr. min.	hr. Min.	
1	1548	0·28	0·180	.	.	0 55	1 5	Death in 1 hour 19 minutes. There were very frequent tetanic attacks and nearly continuous slight trembling.
2	1530	0·25	0·163	8	16	0 36	1 36	Recovery nearly complete in 6 hours. The convulsions were never severe.
3	Same	0·28	0·183	10	20	0 35	None; simply tremors	Recovery in 5 hours.
4	Same	0·3	0·196	.	.	.	.	Recovery in 6 hours after very severe tetanus.
5	Same	0·35	0·228	.	.	0 30	2 0	Death in 2 hours 18 minutes.
6	1530	0·3	0·196	3	35	0 35	3 5	Death in 3 hours 45 minutes. The convulsions were very severe, and consisted of almost continuous clonic spasms, with frequent tonic spasms. Died quietly in state of exhaustion.
7	1445	0·3	0·207	6	15	0 9	2 15	Death in 3 hours 35 minutes. Symptoms as in Expt. 6.
8	1757	0·3	0·170	6	38	0 38	1 8	Death in 1 hour 43 minutes. Symptoms as in Expt. 6.
9	1614	0·3	0·185	.	.	1 20	None. Only tremors and slight startings.	Recovery in about 6 hours.
10	Same	0·35	0·216	.	.	.	.	Recovery in about 5 hours. Had marked tetanus.
11	Same	0·[illegible]	0·247	.	.	.	.	Death in 3 hours 25 minutes. Symptoms as in Expt. 6.

place after 0·13 to 0·46 grm. per kilo. Distinct narcosis occurred in medium-sized rabbits after 5 milligrms., smaller amounts than this having a scarcely perceptible effect. The smallest dose with which an increase of reflexes was obtained was 0·15 grm. (see Tables).

Table showing the Effects of Small and Medium Doses of Morphine Hydrate on Rabbits.

No.	Weight of Rabbit in Grms.	Dose in Grms.	Dose per Kilo. Body Weight.	Effects.
1	907	0·0025	0·0027	Scarcely perceptible drowsiness. Not much change in demeanour.
2	1692	0·005	0·003	Slight narcosis observed 18 minutes after administration, and lasted 3 hours. Respiration fell about two-thirds. Ears hyperæmic and blood-vessels dilated.
3	...	0·01	...	Light narcosis in 14 minutes, and lasted 4 hours. Respiration fell from 30 to 4 per 10 seconds. Pupil became somewhat smaller.
4	1650	0·02	0·012	Light narcosis observed in about 10 minutes, and lasted 6 hours. Respiration fell from 30 to 6 and 4 per 10 seconds. Pupil somewhat smaller.
5	1304	0·05	0·038	Narcosis marked in 4 minutes, and lasted all day. It was deep, and animal could hardly be roused. Pupils were extremely small all the time. Respiration fell from 22 to 4 per 10 seconds.
6	1420	0·1	0·07	Deep narcosis set in after 2 minutes and lasted all day. Pupils extremely small all the time. Respiration fell from 25 to 3 per 10 seconds. When severely pinched only made a slight movement. There was no increase reflexes.
7	907	0·15	0·16	Deep narcosis 1 hour 18 minutes after administration. There was slight increase in reflexes, which passed off very soon, the narcosis remaining deep. The pupil was at first very much contracted, but 17 minutes after administration became rather larger and remained so. It never dilated more than to a small medium size.
8	1304	0·2	0·15	There was rapidly deep narcosis. 38 minutes after administration pupil had dilated to large medium size and remained so. In 2 hours 28 minutes there was slight increase reflexes and animal was restless and gnashing its teeth. Respiration fell from 15 to 5 per 10 seconds. Effects lasted all day.

Physiological Action of Codeine.

(Methylmorphine $C_{17}H_{17}NO.OH.OCH_3$).

The action of codeine obtained directly from opium has been investigated by many experimenters (see Bibliography). We found that artificial codeine prepared from morphine has identically the same action as the natural product, and hence a comparatively small number of experiments was sufficient for our purpose.

Its action is exerted on the central nervous system, and has a close generic resemblance to that of morphine. Schrœder and others have pointed out that it causes an evident narcotic stage, followed by a condition of increased reflex excitability which amounts to tetanus if the dose be large enough. The narcotic stage is, however, shorter and much less deep than with morphine, and if large doses be given to animals is often hardly noticeable or quite absent.

The narcotic action of codeine on man is much feebler than that of morphine, and only in one case (a child) has its tetanising effect been observed.

Its paralysing influence on motor nerves in frogs has been pointed out by several observers, and is very much greater than that of morphine. In frogs the heart is slowed; in Mammalia blood pressure is not affected, and the pulse-rate only very slightly. This is probably due to depression of the cardiac motor ganglia (Schrœder). In dogs and cats it produces marked vomiting and diarrhœa, much more so than morphine.

Frogs.—Our own experiments on frogs confirmed Schrœder's to a large extent. We found that very small doses, such as 0·0013 grm. had no apparent action at the time, although next day there was a very faint increase in the reflexes. Larger doses of 5 milligrms. and upwards caused a characteristic stage of depression with diminished reflexes. This is not deep, lasts but a short time, and is followed by increased reflex. When the dose was 0·01 grm. or more, the narcotic stage was followed by tetanus.

Schrœder states that if as much as 2 or 3 centigrms. be given, the narcotic stage is never developed, but that tetanus comes on almost at once. This, however, depends entirely on the rate of absorption. We found that much larger doses (even 0·06 grm.) occasionally had

marked narcosis preceding the tetanus, while 2 centigrms. might give tetanus almost at once. It is clearly a question of rapidity of absorption.

With the smaller doses the tetanus is exactly like that of strychnine; but with larger amounts there is great tendency to exhaustion, just as with morphine, so that soon no tetanic spasm can be induced, the frog responding to stimulation by a mere twitch. This condition depends partly on depression of the cord and partly on depression of motor nerves. When the codeine is injected directly into the aorta tetanus occurs at once. Thus,

Expt. 6.—Frog pithed; body ligatured, with exception of lumbar nerves.

12.22.—0·005 grm. codeine in ½ cub. cent. water and acetic acid (q.s.) per aortam.

12.23.—Violent tetanus.

12.28.—Tetanus is becoming less violent, but is still marked.

12.45.—Is now a good deal exhausted, but still gives a violent jerk on stimulation.

1.0.—No response on stimulation. The cord is quite exhausted. The brachial nerves (unprotected) are quite excitable at 180 mm., the sciatic nerves (protected) also excitable at 180 mm.

Experiments made in the same way with 1, 2, 3, and 5 centigrms. gave similar results, but exhaustion ensued more rapidly, and, especially with the larger doses, the motor nerves were completely paralysed, or very much depressed in excitability. When small doses such as ½–2 milligrms. are administered by the aorta, there occurs a stage of depression, followed by an increase of reflexes, just as in the case of morphine.

With regard, therefore, to the action of codeine on the cord, we find (as with morphine) that small doses depress its reflex and conducting power, while larger ones cause tetanus. If a large dose be thrown at once into the substance of the spinal cord by injecting it directly into the aorta, then no preliminary narcotic stage occurs, but an increase of reflexes from the very first.

The action of codeine on the motor nerves in frogs is very distinct. When the codeine is injected directly into the vessels it occurs very rapidly, but if a large dose (about 5 centigrms.) be given subcutaneously, paralysis or very marked depression occurs

only after some hours. Thus after 6 centigrms. it was five hours before the sciatics became completely paralysed, the heart still continuing to beat. Much smaller doses, however, cause very evident depression. The exhaustion of the nerve terminations, consequent on the prolonged tetanus, also contributes to the paralysis, for if one sciatic nerve be cut before administering the codeine, we found that it did not become so much paralysed as the other which was left intact.

A dose of 2 centigrms. and upwards was fatal in most cases.

Rabbits.—The action on rabbits is essentially similar to what we have seen in frogs. The narcosis is slight, the animal can easily be aroused, and when the dose is increased, this, instead of deepening the narcosis, causes a condition of increased reflex excitability. The animal in consequence gives frequent starts, or may have tetanus. As a result of the continual disturbance, the narcosis becomes distinctly lighter, although it does not wholly disappear.

The most typical effects can be got by giving the alkaloid in divided doses.

A careful comparison of the results of previous experimenters shows that the minimum lethal dose varies from 0·06 to 0·1 grm. in rabbits. When such large doses are given at once, the narcotic stage is very ill defined or entirely absent, the tetanus supervening almost at once.

The effects of small doses are seen in the accompanying Table.

Action of Small Doses of Codeine on Rabbits.

No.	Weight of Rabbit in Grms.	Dose in Grms.	Dose per Kilo. Body-Weight.	Effects.
1	1530	0·005	0·003	No effect.
2	1445	0·01	0·006	Scarcely perceptible drowsiness.
3	1530	0·02	0·013	Slight drowsiness, lasting 1½ hour. Slight slowing of respiration.
4	1587	0·02	0·012	Same as Experiment 3.
5	1304	0·03	0·023	Very distinctly drowsy for over two hours. Sat up all the time; respiration unaltered.
6	1644	0·03	0·018	Was drowsy for over two hours. Lay on belly; pupils small; respiration unaltered.

From the foregoing it is evident, as Schrœder and others have previously pointed out, that the action of codeine is qualitatively very similar to that of morphine. Its tetanising action and its action on motor nerves are very much greater, while its toxic effect is at least about three times larger. On the other hand, its narcotic power is about four times less than that of morphine (in rabbits). In man the narcotic effect is probably even much more diminished, but exact experiments are wanting to determine the extent.

Ethylmorphine (Codethyline of Grimaux and Bochefontaine).

This compound is the analogue of codeine, being the ethyl ether of morphine, whereas codeine is the methyl ether. We prepared the base by treating together morphine, soda, and ethyl iodide, in molecular proportions in alcoholic solution. The base was further separated and purified in substantially the same way as described under methocodeine. The hydrochloride was found to be very soluble in water, which fact interfered with its purification from that menstruum. From the hydrochloride, which had been recrystallised both from water and from alcohol, the chloroplatinate was prepared.

(*a*) ·433 grm. (dry in water-bath) gave on ignition ·0822 grm. Platinum = 18·98 per cent.

(*b*) ·3098 grm. (dry at 140° C.) gave ·0588 grm. Platinum = 18·97 per cent.

$$2[C_{17}H_{18}(C_2H_5)NO_3.HCl].PtCl_4 = 18{\cdot}97 \text{ per cent. Pt.}$$

The alkaloid itself was prepared from the solution of its muriate by precipitation with ammonia, and recrystallisation from alcohol; by which process it is obtained in well-defined prismatic crystals. In the air-bath the crystals fused at, or near, 190° C.

Physiological Action of Ethylmorphine.

The only observations on the pharmacology of this substance are contained in a short paper by Bochefontaine, who obtained it from Grimaux. He found that in frogs doses of 0·007 to 0·012 grm. caused violent tetanic convulsions like those of strychnine. Rabbits died after 0·11 grm. subcutaneously, the symptoms being again violent tetanus. He considers that its action is the counterpart of the strychnine action.

Our experiments, while confirming those of Bochefontaine as regards the effect of large doses, have led us to the conclusion that ethylmorphine has an action identical with that of codeine (methylmorphine), and that if the dose be properly chosen one can easily distinguish in animals a narcotic and a tetanic stage, just as with the other members of the group. Bochefontaine used too large doses, thereby completely masking the narcotic effect. It seems to be indifferent, therefore, whether a molecule of methyl or ethyl be introduced so long as they replace the same H atom in morphine.

In our experiments we used the hydrochlorate of ethylmorphine, which is quite soluble in water.

Frogs.—With doses of 0·005 – 0·025 grm. the narcotic stage is well marked; the animal becomes lethargic, the reflexes are diminished, movements are not so active, respiration is less frequent, and the pupils diminish in size. With the smaller doses this is followed by an increase in reflexes, with the larger by tetanus. As the dose increases the narcotic stage gets shorter and less pronounced, and the tetanus supervenes more rapidly. Exhaustion follows just as with codeine. The convulsions are spinal, and occur or continue after division of the spinal cord at the atlanto-occipital membrane.

The pupil is small during the first stage, but dilates as the reflex excitability increases.

During the poisoning the motor nerves gradually become impaired in excitability. To avoid repetition we may simply state that they are affected just as with codeine.

With doses of 0·01 – 0·015 grm. the heart is considerably slowed, and the strength of the beats lessened.

Direct injection of a solution into the aorta (0·01 grm.) brought on tetanus at once without previous depression.

The minimum lethal dose for a frog is generally about 2 centigrms., although smaller doses sometimes prove fatal, probably from exhaustion after the tetanus.

The following experiment shows its action :—

Expt. 8.—Frog, 24 grms.

3.8.—0·01 grm. ethylmorphine hydrochlorate in ½ cub. cent. water subcutaneously.

3.12.—Pupil much smaller; rather sluggish.

3.18.—Pupil still smaller; more sluggish; jumps very heavily; placed on back, struggles but cannot recover its position. Reflexes are much diminished; only responds slightly to pinching toe.

3.25.—Lies on back without struggling, otherwise about same.

3.45.—Reflexes increased. Frog is having almost continuously slight tetanic jerks. Pupils are very dilated. Position is that of a frog poisoned with strychnine.

4.5.—The slightest touch causes one tetanic spasm, leaving the frog much exhausted.

4.50.—Same, but becoming more exhausted.

The duration and onset of the two stages vary a good deal even with the same dose, this being doubtless due to differences in the rate of absorption of the poison.

Rabbits.—Here also the action is similar to that of codeine. When small doses are given the animal becomes drowsy and lethargic or sleeps quietly, respiration is slowed, while the heart and pupil are but slightly affected. It can always be easily roused.

When larger doses are administered there is considerable narcosis, intermingled after a short time with increased reflexes, which may proceed to clonic and tonic convulsions. During these the pupil dilates.

When a lethal dose is given the animal quickly dies in tetanus, just as with strychnine. About 0·1 grm. is the minimum lethal dose for medium-sized rabbits. The following experiment, along with the subjoined Table, gives a tolerably correct idea of its general action:—

Expt. 9.—Rabbit, 1502 grm. Ht., 32; R., 27 in 10 secs.

10.54.—0·08 ethylmorphine hydrochlorate in 2 cub. cent. water subcutaneously.

11.0.—Quieter, resting chin on table; no increase reflexes. H., 27; R., 25.

11.3.—Alarmed, jerks at least noise.

11.5.—Had a clonic convulsion (trembling).

11.7.—Is a good deal narcotised, but reflexes are increased, and starts spontaneously or on stimulation.

11.20.—Pupil dilated. *In statu quo.*

11.47.—Lying on belly narcotised, but can be easily aroused. Often gives a slight spontaneous start. H., 27; R., 22.

12.0.—Narcosis deeper; reflex excitability is diminishing. H., 26; R., 12.

1.30.—Lying on belly with head on table; is easily aroused but relapses at once into somnolence. Sometimes gives a faint twitch. H., 30; R., 8.

3.30.—Has gradually recovered, and, although still very sleepy, is now sitting up.

5.30.—Almost completely recovered, but still sleepy and lethargic. H., 33; R., 13.

Action of Hydrochlorate of Ethylmorphine on Rabbits.

No.	Weight of Rabbit in Grms.	Dose in Grms.	Dose per Kilo. Body Weight.	Remarks.
1	1247	0·01	0·008	Was slightly drowsy for 3 hours.
2	963	0·02	0·021	Very distinctly narcotised for about 4 hours. Respiration fell from 29 per 10 seconds to 4. Ht. from 34 to 26. Was always easily roused. No increase reflexes.
3	963	0·02	0·021	Same as in Experiment 2.
4	1247	0·05	0·04	Lay as if deeply asleep for 2 hours, and then gradually recovered. Respiration fell from 19 to 11. Ht. and pupil remained unaltered. Could always be easily roused.
5	1502	0·08	0·053	Experiment 9 in text.
6	1190	0·08	0·066	Almost same as 5.
7	963	0·01	0·103	Was at first narcotised, but in 6 minutes had clonic spasms, and in 9 minutes violent tetanus, during which it died. *Post mortem* was negative.

The action of ethylmorphine is therefore, so far as our experiments permit us to judge, exactly similar to that of codeine, and stands in the same relation to morphine.

Amylmorphine.

This compound was formed in the same manner as the methyl and ethyl derivatives, by allowing morphinate of soda and amyl chloride to react together in alcoholic solution, with subsequent evaporation and extraction with chloroform. On converting the base into muriate the salt readily crystallised.

Of the chloroplatinate ·3115 grm. gave ·055 grm. Pt = 17·68 per cent.

$$2[C_{17}H_{18}(C_5H_{11})NO_3]PtH_2Cl_6 = 17{\cdot}48 \text{ per cent. Pt.}$$

Physiological Action of Amylmorphine.

The hydrochlorate of amylmorphine was used, but as the results showed its action to be very similar to, if not identical with, that of methyl- or ethylmorphine, it is unnecessary to go into further details.

Acetylmorphine.

The acetyl derivatives of morphine were first investigated by Wright. In our first experiments we prepared acetylmorphine by the action of boiling glacial acetic acid on morphine for several hours, but we found on analysis of the substance prepared in that way that it evidently contained morphine. Better results were obtained by heating together at 100° C. equivalent proportions of anhydrous morphine and acetic anhydride. After reacting for an hour, water was added, and ammonia in slight excess. The mixture was then shaken up with ether, in order to remove the acetylmorphine, and the ethereal solution agitated with hydrochloric acid, which caused the acetylmorphine to separate as hydrochloride. This salt is rather sparingly soluble in water. The chloroplatinate dried at 100° lost no weight further by drying at 130°.

·124 grm. gave by ignition ·023 grm. Pt = 18·54 per cent.

$$2[C_{17}H_{18}(C_2H_3O)NO_3.HCl]PtCl_4 = 18{\cdot}48 \text{ per cent. Pt.}$$

The salt, as prepared by the action of glacial acid on morphine, gave a chloroplatinate, yielding 18·20 per cent. Pt.

Physiological Action of Acetylmorphine.

The only previous experiments with this alkaloid are those of F. M. Pierce, who obtained it from C. R. A. Wright. He made three observations on dogs and two on rabbits. These were sufficient to demonstrate that the action has a general resemblance to that of codeine, but they do not show what relationship qualitatively and quantitatively exists between acetylmorphine and the other members of the morphine group.

Our own observations were made on frogs, rabbits, and dogs. As

the hydrochlorate is not a very soluble salt, we used a solution of acetylmorphine in water and acetic acid.

Frogs.—Doses of 5 milligrms. caused restlessness, but no characteristic symptoms. When 0·01 grm. is exhibited the frog shows perfectly the two stages, although neither the narcotic nor the tetanic condition is very pronounced. With 0·015–0·025 grm. there is marked lethargy and depression of reflexes, followed in a very short time by violent tetanic spasms. The larger the dose the more rapidly does tetanus ensue.

If a solution (2–3 centigrms.) be injected directly into the aorta tetanus ensues at once without any preliminary depression of reflexes. Acetylmorphine paralyses or depresses motor nerves to very much the same extent as codeine does.

The heart is markedly slowed. After 0·01 grm. the rate fell from 56 to 44 per minute.

The following experiments show its general action :—

Expt. 10.—Frog, 26 grms. R., 20 in 10 seconds.

2.40.—0·01 acetylmorphine subcutaneously.

2.48.—Very dull and lethargic. Pupils smaller. R., 17.

3.0—Lies on back if placed there. Jumps very clumsily. Pupils small. R., 8. Reflexes diminished.

3.42.—In same condition.

4.22.—Pupil now very large; slight increase in reflexes. Is active and lively.

5.10.—About same. Narcosis has now passed off.

Next day, active and lively. Slight increase of reflexes.

Expt. 11.—Frog, 28 grms. R., 18 in 10 seconds.

10.59.—0·025 under skin of back.

11.2.—Much more sluggish; pupil smaller. R., 14.

11.10.—R. ceased. Lies on back; pupil very small; responds only to severe pinching.

11.16.—Is still much narcotised, but each irritation brings on a tetanus. Pupil remains small during the spasm.

11.20—Lying in strychnine position. Tetanus occurs spontaneously and on irritation.

11.35.—Is very exhausted after each spasm, so that irritation does not bring on another until after an interval. Corneal reflex gone.

12.10.—Very much exhausted, and on irritation gives simply a jerk.

12.30.—Lying as if dead. Only sign of life is beating of heart, 10 in 30 seconds.

Rabbits.—In rabbits the narcotic action is developed after the very small dose of one milligrm. The stupor is by no means profound, but the animal remains drowsy for some hours; the pupils are small, and the respirations are diminished in frequency. When the dose is gradually increased there occurs marked narcotism, with great weakness in the limbs, and remarkable slowing of the respiration. The respiration may be so much slowed that the blood is not properly aerated and dyspnœa results. When the dose is increased to 0·15–0·25 grm. the narcotic stage is much shortened, the increase of reflex excitability comes on more quickly, and proceeds to clonic or tonic spasms and death. The heart becomes very feeble towards the end.

Just as with other members of the morphine group, the narcotised condition of the brain does not wholly pass off during the tetanic stage, although it seems to be a good deal lessened, probably by the constant irritation and state of disquietude in which the animal is kept by the spasms.

Dogs.—In a spaniel weighing 13 kilogrms., 1 milligrm. subcutaneously caused slight drowsiness; larger doses (1–4 centigrms.), while having a more marked narcotic effect, had a distinct tendency to cause nausea and looseness of the bowels.

Expt. 12.—Rabbit, 1474 grms. Ht., 27; R., 13 in 10 seconds.

1.22.—0·1 acetylmorphine subcutaneously.

1.24.—Head on table. H., 22, irregular and intermittent; R., 2.

1.30.—H., 24, regular; R., 2. Holds head in air as if suffering from dyspnœa. Pupil slightly smaller.

1.37.—H., 27; R., 2 in 15 seconds; head still held up. Lying on belly, with legs spread out.

1.45.—Reflex excitability is increased.

1.55.—H., 27; R., 3. Noise or touch causes slight start.

2.40.—H., 24; R., 5. Reflexes very little increased. Legs very weak; cannot stand.

4.10.—H., 26; R., 6. Is still a good deal narcotised. Reflexes not increased now.

7.30.—H., 26; R., 4. Is still a good deal narcotised. Next day, well.

Expt. 13.—Rabbit, 935 grms. H., 29; R., 22 in 10 seconds.

9.56.—0·15 acetylmorphine in 10 seconds.

9.58.—H., 25; R. scarcely perceptible. Lying on belly, with head on table. Pupil extremely small.

10.2.—H., 28; seems scarcely to breathe. Evidently suffering from dyspnœa, as it holds its head backwards and upwards. No increase reflexes. Pupil still very small.

10.10.—H., 29; R., 3. Slight increase reflex excitability.

10.15.—Had slight tetanic jerk.

10.25.—Has clonic spasms continuously, trembling and jerking. H., 25; R., 5. Pupils medium.

10.32.—Clonic spasms more violent.

11.0.—H., 27; R., 6. Spasms not quite so frequent.

11.45.—Tendency to spasm is rapidly passing off. Lying on belly with chin on table; legs are sprawled out, and so weak that rabbit cannot move itself. H., 28; R., 8.

12.30.—Has lain quietly. H., 29; R., 3. Pupil medium. Stimulation still causes slight spasm.

2.5.—H., 29; R., 6. Lying on side; still gives occasionally a slight jerk. Resp. is extremely faint and hardly perceptible.

2.20.—Died quietly from gradual failure of respiration.

Post-mortem.—Appearances of asphyxia.

In acetylmorphine the same hydrogen atom of morphine has been replaced as in methyl- or ethylmorphine, but by an acid radical (acetyl) instead of an alkyl radical. Its action is much nearer to that of codeine than to that of morphine.

Compared with morphine, its power of causing tetanus is much increased, while its narcotic effects, although visible after smaller doses, are not nearly so deep. As soon as we increased the dose for the purpose of deepening the narcosis, the increase of reflexes was developed and disturbed the narcotic effect. The depressant action which it exerts on respiration is much greater than that of morphine.

Compared with codeine, an equal narcotic effect is induced in rabbits by about one-tenth of the dose, while about a three times larger dose is necessary to cause tetanus. Qualitatively, therefore,

we find that acetylmorphine closely resembles the other members of the group, differing chiefly quantitatively and in some slight matters of detail,—that is to say, it acts on the same systems and organs of the body, and affects them in a similar manner.

Action of Acetylmorphine on Rabbits.

No.	Weight of Rabbit in Grms.	Dose in Grms.	Dose per Kilo. Body Weight.	Effects.
1	1464	0·001	0·0006	Was distinctly drowsy for 2 hours. Respiration fell from 23 per 10 seconds to 11. Heart unchanged.
2	849	0·001	0·0011	Drowsiness began in 5 minutes and lasted about 2 hours. Lay during this time as if soundly asleep. After 9 minutes respiration had fallen from 19 to 4 per 10 seconds. Heart remained same.
3	1350	0·0025	0·0016	Same as 2.
4	793	0·0025	0·003	In 10 minutes after administration lay on belly with chin on table as if soundly asleep, and remained so for 3 hours, then slowly recovered. Respiration fell from 21 to 6 in 10 seconds. Pupils were small. Heart remained same.
5	1020	0·005	0·005	In 2 minutes was drowsy, and pupils smaller; in 4 minutes respiration had fallen from 22 to 5 per 10 seconds and pupil very small; cannot stand up; in 10 minutes respiration 2; was considerably narcotised for 5 hours and gradually recovered.
6	930	0·01	0·011	Was drowsy in 2 minutes; pupil very small in 4 minutes, and respiration fell from 19 to 5. Lay for 5 hours as if deeply asleep; difficult to rouse.
7	1133	0·05	0·043	Was considerably narcotised for 8 hours. No increase in reflexes.
8	1474	0·1	0·067	Experiment 12 in text.
9	935	0·15	0·16	Experiment 13 in text.
10	1077	0·25	0·23	Same as Experiment 9.

Diacetylmorphine.

This compound was prepared by the action of acetic anhydride on anhydrous morphine, in the proportion of 25 parts of the former to 30 parts of the latter. When a smaller amount of acetic anhydride was used, the product was found to be mixed with the monacetyl derivative. After digesting the morphine and acetic anhydride together for several hours on the water-bath, the product

was evaporated, taken up with water, and slight excess of sodium carbonate added. The mixture was then shaken up with chloroform, which on evaporating left a crystalline residue. This was dissolved in hot alcohol, the solution left to crystallise, and the crystals separated and pressed. From these a chloroplatinate was prepared, and a portion of it dried in water-bath.

·255 grm. gave ·043 Pt. = 16·86 per cent.

Portion dried in air-bath at 130° C.

·2985 grm. gave ·0505 Pt = 16·91 per cent.

$2[C_{17}H_{17}(C_2H_3O)_2NO_3.HCl]PtCl_4$ = 17·13 per cent.

Physiological Action of Diacetylmorphine.

Pierce also made a few experiments on dogs with this alkaloid, and obtained much the same results as with acetylmorphine.

Our experiments were made on frogs, rabbits, and dogs with a solution of diacetylmorphine in acetic acid and water.

We found its action to be exactly similar to that of acetylmorphine, and hence a detailed account is superfluous. It seems to us to be slightly more active, both as a narcotising and tetanising agent, but otherwise there is no difference. The lethal dose is about the same, and the same very remarkable slowing of the respiration was observable.

For purposes of comparison we give notes of some of the experiments.

Expt. 14.—Rabbit, 1530 grms. H., 34; R. 19 in 10 seconds.

12.48.—0·01 diacetylmorphine subcutaneously.

12.50.—Drowsy.

12.52.—H., 27; R., 3. Pupils much smaller; chin resting on table, exactly as if sound asleep.

12.58.—H., 29; R., 1. Blood in ear vessels looks very venous.

1.30.—H., 30; R., 2. Very narcotised, no increase reflexes.

2.0.—H., 33; R., 5.

2.30.—H., 29; R., 2 Lying as if very sound asleep. Easily roused, but falls asleep again almost at once.

3.30.—H., 30; R., 3. About same.

4.30.—H., 29; R., 6. Still rather drowsy; can move about, but legs are very shaky and weak.

Expt. 15.—Rabbit, 840 grms. H., 28; R., 15 in 10 seconds.

10.53.—0·12 grm. diacetylmorphine subcutaneously.

10.55.—H., 29; R., 2. Pupils smaller; lying on belly.

11.0.—H., 30; R., 3. Increase of reflexes marked; pupil large.

11.6.—Violent tetanic convulsion, followed shortly by two more.

11.20.—Lying on belly; frequently starts violently, and sometimes has tetanus. H., 28; R., 3.

12.30.—Clonic spasms have been nearly continuous; head, neck, and legs are jerked almost without ceasing, and the teeth are gnashed. Touch causes a start. H., 28; R., 3.

2.30.—Has been same, and has had several violent tetanic attacks.

2.50.—Lying on side, having frequent tetanic attacks, very nearly dying in each. Pupil large. H., 12; R., 4.

3.20.—Died during a convulsion.

Post-mortem examination was negative, except the ordinary appearances of death by asphyxia.

Action of Diacetylmorphine on Rabbits.

No.	Weight of Rabbit in Grms.	Dose in Grms.	Dose per Kilo. Body Weight.	Effects.
1	1474	0·001	0·0006	Was drowsy for about 2 hours; respiration fell from 19 to 9 per 10 seconds.
2	750	0·001	0·0013	Was drowsy for 3 hours; respiration fell from 17 to 8.
3	1530	0·005	0·0032	Was drowsy in 2 minutes; in 4 minutes lay down with chin on table; pupils very small; in 14 minutes respiration was 3 in 10 seconds; weak on legs and cannot stand. Remained considerably narcotised for 4 hours.
4	1530	0·01	0·065	Experiment 14 in text.
5	935	0·02	0·021	In 6 minutes respiration fell from 18 to 2 per 10 seconds. Lay as if deeply asleep; pupils small; legs very weak. Remained so for 6 hours.
6	1530	0·04	0·026	Became deeply narcotised in a few minutes. Temperature, pulse-rate, and respiration fell; great weakness in legs. Remained so all day.
7	954	0·05	0·052	Was deeply narcotised all day.
8	840	0·12	0·14	Experiment 15 in text.
9	1530	0·15	0·098	Much the same as in Experiment 8, but recovered.

In a dog weighing 7400 grms. the administration of 5 milligrammes caused drowsiness for three hours. There was consider-

able fall of heart-rate and respiration, salivation and a tendency to diarrhœa. The narcosis was by no means deep. Larger doses, of 2 and 5 centigrammes, caused symptoms very similar to what Cl. Bernard has described as occurring in dogs after codeine.

Benzoylmorphine.

This body was originally prepared by the action of benzoyl chloride on morphine. The process has the disadvantage in that, if an excess of alkaloid be used, there is apt to be a very small yield of the desired product, while, on the other hand, if excess of benzoyl chloride be employed, there is sure to be formation of a certain amount of dibenzoylmorphine. It occurred to us that the method which succeeded with alkyl chlorides might also be applied with chlorides of acid radicals. Accordingly 30 grms. of morphine were dissolved in alcohol with 4 grms. of caustic soda, and 14 grms. of benzoyl chloride added. The mixture was allowed to stand in the cold for twelve hours, then heated to boiling and evaporated. Water and a little sodium carbonate were added to the residue, the mixture shaken up with chloroform, filtered, and the chloroform separated and evaporated. The residue so obtained was purified by crystallisation from hot alcohol. From the product a chloroplatinate was prepared and dried at 130°. Of this ·373 grm. gave ·0605 grm. Pt. = 16·22 per cent.

$$2[C_{17}H_{18}(C_7H_5O)NO_3.HCl]PtCl_4 = 16{\cdot}48 \text{ per cent. Pt.}$$

The base gives no blue coloration with ferric chloride. As observed in the air-bath, it fused at 168° C. When converted into hydrochloride, the strong aqueous solution showed no signs of crystallisation even after several days. From these results it is evident that the above method of double decomposition proceeds with benzoyl chloride as it does with methyl chloride.

Physiological Action of Benzoylmorphine.

The physiological action of this substance has not been previously investigated. After a few experiments it became evident to us that the action is practically identical with that of acetylmorphine. In frogs 5 milligrms. caused a slight degree of narcosis, followed by increase of reflexes. One centigrm. induced marked narcosis,

followed by tetanus in a quarter of an hour or more, while 2 centigrms. was a lethal dose.

The action on rabbits can be seen from the subjoined Table.

Filehne has stated that benzoylmorphine is a local anæsthetic, but after a number of experiments on ourselves and on animals we are unable to confirm his results.

Benzoyl, like acetyl, is an acid radical, and our results seem to show that it is of little consequence as regards action which of them is introduced, so long as they replace the same hydrogen atom in morphine.

Action of Benzoylmorphine on Rabbits.

No.	Weight of Rabbit in Grms.	Dose in Grms.	Dose per Kilo. Body Weight.	Effects.
1	1500	0·001	0·00066	Lay for 2½ hours as if gently asleep; respiration fell from 17 to 10 per 10 seconds.
2	1304	0·005	0·0039	Lay as if asleep for 6 hours. Was a good deal narcotised, but could always be easily roused. In 5 minutes respiration fell from 16 per 10 seconds to 11, and in 11 minutes to 3.
3	1560	0·01	0·0064	Was deeply narcotised for 6 hours. Respiration rapidly fell from 24 to 2 in 10 seconds. No increase reflexes.
4	1560	0·1	0·064	Was at first much narcotised, but in 20 minutes reflexes had greatly increased and in 27 minutes had tetanus. After lasting for about 6 hours, the tetanic condition passed off, but the animal remained deeply narcotised for many hours afterwards.

Dibenzoylmorphine.

10 grms. of anhydrous morphine and 20 grms. benzoylchloride were together heated in a sealed tube placed in a water-bath for seven hours. The contents of the tube were then evaporated, the residue treated with hot water, and excess of ammonia added. The whole was now shaken up with chloroform, and the latter evaporated. There were no signs of crystallisation; wherefore the substance was dissolved in ether, and the ether driven off, but the solution refused to crystallise. Its chloroplatinate, dried at 140° C., was ignited. ·2565 grm. gave ·0365 grm. Pt. = 14·23 per cent.

$$2[C_{17}H_{17}(C_7H_5O)_2NO_3 \cdot HCl]PtCl_4 = 14{\cdot}03 \text{ per cent. Pt.}$$

It was attempted to further purify the compound by treating with alkali, washing the precipitate, and dissolving the same in alcohol. Crystals formed very slowly, were rather dark in colour, and gave a platinum salt, which yielded only 13·5 per cent. of metal on ignition; whence it is probable that the dibenzoyl derivative is somewhat prone to decomposition, and is therefore not a very satisfactory compound to work with.

Physiological Action of Dibenzoylmorphine.

So far as we could judge, the action of this substance given by the stomach resembles the action of benzoylmorphine. It was impossible to carry out exact observations, as the attempt to form a soluble salt of dibenzoylmorphine by adding acid resulted in the compound being broken up and benzoic acid precipitated.

Methylmorphium Chloride (Morphine Methochloride).

When methylchloride is brought in contact with morphine in solution, preferably under pressure, direct combination of the two compounds takes place. If morphine methochloride were a compound strictly analogous to morphine hydrochloride, we should expect that the former would yield, on treatment with potash, potassium chloride, methyl alcohol, and morphine. As a matter of fact, however, we obtain potassium chloride and methylmorphium hydroxide (morphine methohydroxide), no precipitate of morphine being obtained under any circumstances. When a morphine or methylmorphine salt is heated in a sealed tube with hydrochloric acid, apomorphine is the chief product of the reaction. On heating methylmorphium chloride in the same manner, we have found that no apomorphine is formed. The compounds formed by the union of an alkaloid with an alkyl haloid were described by Crum Brown and Fraser as methylmorphium iodide, ethyl strychnium chloride, &c., to indicate that these are analogous to the corresponding ammonium compounds, the alkaloids from which they are derived being analogous to ammonia. The compounds in question are now more usually described as morphine methiodide, strychnine ethochloride. &c., these names being considered more systematic from a chemical point of view, but the nomenclature adopted by Crum Brown and Fraser is probably the better, from a pharmacological standpoint,

A curious error has crept into several text-books referring to these additive compounds of alkaloids, an error which amounts to a misrepresentation of the investigation of Crum Brown and Fraser. In these books the compounds are referred to as substitution compounds, in which hydrogen has been replaced by an alcohol radical, whereas they are addition compounds. Stolnikow, in his paper on morphine, and Boehm (Herzgifte, p. 3), have made this same mistake.

The methylmorphium chloride employed in our experiments was prepared as follows. Morphine was dissolved in hot methylated spirit with a proportion of soda, and gaseous methyl chloride passed into the solution. The alcohol was then evaporated, and the codeine extracted by means of chloroform; after which the solution was neutralised with hydrochloric acid, and faint excess of ammonia added. By this process the morphine was separated, and on evaporating the mother-liquor, methylmorphium chloride crystallised out. The salt was purified by several crystallisations from water, and was obtained in colourless, well-defined prisms. The solution gave no precipitate with ammonia or sodium carbonate, and otherwise behaved like a methylmorphium salt.

The salt lost weight by exposure in the water-bath, but even at 120° C. was not completely dried. On raising the temperature to 140°–145° ·4997 grm. lost ·0475 grm.

$$\text{Loss of weight at } 140° = 9{\cdot}505 \text{ per cent.}$$

$$C_{17}H_{19}NO_3.CH_3Cl.2H_2O = 9{\cdot}690 \text{ per cent. } H_2O.$$

The chlorine was estimated in the air-dry salt by dissolving in water, adding nitric acid and nitrate of silver, according to the usual method.

$$\cdot 5385 \text{ gave } \cdot 208 \text{ Ag Cl} = \cdot 0514 \text{ Cl, which is} = 9{\cdot}54 \text{ per cent. Cl}$$

$$C_{17}H_{19}NO_3.CH_3.Cl.2H_2O = 9{\cdot}55 \text{ per cent. Cl.}$$

In a second determination ·621 grm. gave ·239 Ag Cl = ·0591 Cl, which is = 9·51 per cent.

The chloroplatinate was prepared by dissolving the salt in water, adding slight excess of platinic chloride, and washing the collected precipitate with cold water. ·268 grm. of the chloroplatinate (dry in water-bath) was incinerated, and left ·0496 platinum = 18·50 per cent.

$$(C_{17}H_{19}NO_3.CH_3Cl)_2.PtCl_4{\cdot}3H_2O = 18{\cdot}51 \text{ per cent.}$$

Another portion of the chloroplatinate was dried in air-bath at 120° C. ·271 grm. incinerated left ·0528 grm. Pt., which is = 19·48 per cent.

$$(C_{17}H_{19}NO_3.CH_3Cl)_2PtCl_4 = 19{\cdot}50 \text{ per cent. Pt.}$$

Physiological Action of Methylmorphium Chloride.

The only previous investigation into the action of this body is that of Crum Brown and Fraser. Our results confirm theirs in some respects, but differ materially in others, and hence it may be well to give a summary of their experiments. They used the iodide and sulphate of methylmorphium, and performed the following experiments with them :—20 grains of the iodide were given subcutaneously to a rabbit, suspended (but not dissolved) in 2 drachms water; 30 grains were given per os. Dr Fraser took ½ and 1 grain. In no case was any visible effect produced. The iodide is, however, owing to its insolubility, an unsuitable salt for experiment, and would dissolve so very slowly that its effects might be inappreciable. Hence these observations may be left out of account. To get rid of this fallacy, they used the sulphate, which is quite soluble. Doses of 2, 3, 4, 5, and 8 grains gave marked symptoms, while 10 grains was fatal to rabbits in 55 minutes, and 15 grains in 7 minutes.

On frogs they performed four experiments. One grain of iodide of methylmorphium caused paralysis, followed by recovery; 2 grains, paralysis, followed by death. Two experiments made with 1 grain of the sulphate caused death, preceded by paralysis. In neither case was the duration of the symptoms observed. Their conclusions are given as follows:—"It has been proved in a most satisfactory manner that sulphate of methylmorphium possesses no convulsant action; for neither in the experiments we have described in detail, nor in any of the others we performed with this substance, was there any trace of spasmodic action or of exaggeration of the reflex function. It, however, undoubtedly causes hypnotic symptoms. It would therefore seem that sulphate of methylmorphium agrees with morphia in possessing a hypnotic action, but differs from it in producing paralysis and in being free from all convulsant action." They hold that the most prominent action of methylmorphium consists in paralysis of the terminations of motor nerves, and that this is the cause of the general paralysis.

Our results agree with theirs as regards the fact that methylmorphium retains the narcotic action of morphine, but we find that it also retains the tetanising action to a very marked degree. As Crum Brown and Fraser point out, the action on motor nerves is a markedly paralysing one, but, as we shall show, this obscured the tetanus and led them to incomplete conclusions. The four frogs on which they experimented received too large doses, and were not observed for a sufficient length of time to show the proper development of the action of methylmorphium.

Our experiments were made on frogs and rabbits with the chloride of methylmorphium, which is a very soluble salt.

Frogs.—When a small dose of 5 milligrms., or a larger dose, is given subcutaneously to a frog, the symptoms are very much the same in all cases. The animal speedily passes into a more or less flaccid condition, and remains so for a length of time varying with the size of the dose.

On examining more particularly into the causes of this paralysis, and into what parts of the nervous system are affected, we found that these depended greatly on the amount of methylmorphium administered. When a small or medium dose is given, the brain and spinal cord are depressed in very much the same way as by a small dose of morphine, while the motor nerves are left practically unaffected. The depression is followed by slight increase of the reflex excitability.

This is shown in the two following experiments:—

Expt. 16.—Frog, 31 grms. R., 22 in 10 seconds.

11.38—0·005 in ½ cub. cent. water subcutaneously.

11.47.—Is much duller; reflexes much diminished. R., 14.

11.58.—Lies on back if placed there; reflexes much diminished; movements badly co-ordinated. R., 12.

12.4.—Left sciatic nerve exposed, and when stimulated at 220 mm. (Du Bois Reymond Coil) gives marked contraction of muscles.

12.12.—Reacts only by slight twitch when severely pinched with forceps. Sciatic nerve markedly excitable at 220 mm.

1.0—Is quite flaccid; scarcely responds to severe pinching. Left sciatic quite excitable at 200 mm., right sciatic at 180 mm.

2.10.—Recovering; moves about if pinched. Nerves same.

6.30.—Is still rather depressed. Nerves same.

Expt. 17.—Frog, 24 grms. Whole tissues of right thigh ligatured except sciatic nerve.

2.36.—0·005 grm. methylmorphium chloride subcutaneously.

2.40.—Much duller.

2.44.—Lies on back; ceased to respire.

2.48.—No response to severe pinching with forceps. Right sciatic quite excitable at 90 mm.; left sciatic quite excitable at 90 mm.

6.0.—Has been in same condition. Both sciatic nerves are equally excitable at 100 mm.

Next day.—Has recovered greatly; active and lively; slight increase in reflex excitability. Both sciatic nerves are equally excitable.

From these experiments it is evident that the brain and spinal cord are much depressed in activity, the motor nerves being left intact, or nearly so. The paralysis and narcosis are due entirely to the action on the nerve centres.

After larger doses deep narcosis quickly supervenes, with greatly diminished reflexes, indifference to painful stimuli, and contraction of pupils. Much later, complete paralysis or marked depression of the motor nerve termination comes on, but as soon as this has passed off the reflex excitability is seen to be greatly increased.

These results are shown in the following experiment:—

Expt. 18.—Frog, 24 grms.

12.3.—0·01 methylmorphium chloride subcutaneously.

12.10.—Lying quite flaccid; both pupils small; reflexes greatly diminished. When pinched crawls away.

12.15.—Makes very slight response to severe pinching. Both sciatic nerves quite excitable at 120 mm.

12.45.—Faint twitch of muscles when sciatic nerves are stimulated.

1.20.—Sciatic nerves inexcitable to strongest current.

Next day.—Reflexes are slightly increased. Frog active and lively.

Third day.—Reflex excitability is now markedly exaggerated.

In another experiment in which the same dose was given the motor nerves were never completely paralysed, although much depressed in electric excitability.

When 0·02 grm. and upwards is given, there is marked depression of the brain and spinal cord, but very soon (in $\frac{1}{4}$–$\frac{1}{2}$ hour) the motor nerves begin to be impaired, and finally their excitability is entirely abolished. With the smaller of these doses the paralysis of motor nerves passes off next day, and the frog is found to be in a condition of greatly increased reflex excitability, but if the dose be 3 centigrammes or upwards it generally dies before the paralysis has passed off.

Expt. 19.—Frog, 31 grms.

2.20.—0·02 grm. methylmorphium chloride subcutaneously.

2.40.—Has gradually become more and more flaccid; pupils extremely small. Both sciatic nerves paralysed.

Second day.—Sitting up in its natural position. The reflex excitability is greatly increased; pupils large.

Third day.—Found dead.

When the alkaloid is given subcutaneously the reflex excitability is never so much increased as to amount to tetanus, the reason being that doses large enough to cause tetanus kill the animal before the paralysis of motor nerves has passed off. If, however, the motor nerves be protected, and the methylmorphium be then injected into the aorta so as to reach the spinal cord at once, we get immediate violent tetanus, just as we have seen with morphine itself, or with codeine and other morphine derivatives. This proves conclusively that methylmorphium still retains the convulsant action which is simply masked by its paralysing action on motor nerves. The following experiment may serve as an example :—

Expt. 20.—Frog decapitated; body ligatured, except lumbar nerves; right aorta tied; cannula in left aorta.

11.10.—0·03 grm. in 1 cub. cent. water injected into left aorta.

At once there was rigid tetanus, which lasted till 11.21, when it became somewhat relaxed.

11.25.—Each stimulation causes one tetanic spasm.

11.34.—Response much feebler to stimulation.

12.0.—A faint twitch on stimulation. The lumbar nerves are still quite excitable to electric stimulation, hence the exhaustion must be due to paralysis of the spinal cord.

Two centigrammes administered in the same way gave similar results, but small doses such as 5 milligrammes caused depression

of spinal cord, just as we found small doses of morphine or codeine to do. We found 2 centigrammes and upwards to be a lethal dose for frogs. The heart beats well long after paralysis is completely developed.

Rabbits.—On rabbits methylmorphium chloride has the same action as on frogs, but owing to their higher organisation the details cannot be worked out in the same complete manner. When 2½ to 5 centigrammes are given subcutaneously drowsiness and narcotism ensue, apparently without any paralysis of motor nerves. With larger doses there is marked narcotism, probably some depression of motor nerves, and a tendency to increased reflex which is never very pronounced. Probably it is obscured by the depression of the motor nerves. As soon, however, as the dose is large enough to paralyse the motor nerves death occurs rapidly from asphyxia.

The blood-vessels in ears were seen always to dilate enormously.

Expt. 21.—Rabbit, 1653 grms. H., 32; R., 26 in 10 seconds.

12.4.—0·3 methylmorphium chloride subcutaneously.

12.13.—Gait less steady. H., 33; R., 24.

12.19.—Lying down. Trembles, and occasionally gives a slight start; starts if tapped or on hearing a noise; pupils widely dilated.

12.25.—Very flaccid, but gives occasionally a spasmodic jerk; sciatic nerves are much depressed in excitability. H., 28; R., 6.

12.40.—Had an irregular convulsion (probably dyspnœic, not tetanic); corneal reflex very sluggish; pupil dilated; sometimes gives a violent start on stimulation.

1.5.—Sitting up. H., 27; R., 14. Increase in reflexes has completely passed off; still drowsy.

5.0.—Has gradually recovered.

Expt. 22.—Rabbit, 780 grms. H. 28; R., 23 in 10 seconds.

2.36.—0·5 grm. subcutaneously.

2.39.—Chin fallen on table; pupils smaller. R., 12.

2.40.—Lies in any position as if deeply narcotised; gives occasionally a start.

2.42.—R. is difficult; pupil very small; cyanosed.

2.43.—Died quietly. The motor nerves were found inexcitable to electric stimulation, otherwise nothing remarkable.

The minimum lethal dose seems to be about 3 decigrammes for medium-sized rabbits, about the same as the lethal dose of morphine,

but its tendency to paralyse motor nerves probably makes it somewhat more deadly.

Action of Methylmorphium Chloride on Rabbits.

No.	Weight of Rabbit in Grms.	Dose in Grms.	Dose per Kilo. Body Weight.	Effects.
1	1680	0·025	0·015	Slightly drowsy for 2½ hours.
2	Same	0·05	0·03	Was drowsy for more than 4 hours. Respiration fell in 42 minutes from 23 to 12 per 10 seconds.
3	1680	0·1	0·06	Drowsiness and weakness of gait in 8 minutes, and lay down. In 38 minutes there was slight increase in reflexes, which soon passed off, and animal remained drowsy all day.
4	1790	0·2	0·112	Drowsy all day. 51 minutes after administration there was slight increase of reflexes, which soon passed off.
5	1160	0·2	0·172	About same as Experiment 7.
6	1790	0·3	0·167	Same symptoms as in Experiment 7, but died in 1 hour 40 minutes from paralysis of motor nerves.
7	1653	0·3	0·180	Experiment 21 in text.
8	Same	0·5	0·302	Same as in Experiment 9.
9	780	0·5	0·65	Experiment 22 in text.

From the foregoing experiments it is obvious that the addition of a molecule of methylchloride to morphine does not alter its physiological action so profoundly as has been stated. The paralysing action on motor nerves is considerably increased and the narcotic action is lessened, but qualitatively its effects on the animal organism remain similar to those of morphine.

Looking at the matter from a chemical point of view, this is what one would expect. Here, just as in the case of methylmorphine or acetylmorphine, &c., we have not made any profound chemical change in the intimate structure of the morphine molecule. We have simply added on a radical to one of its outlying groups, and although the addition has altered the quantitative action it has left untouched the qualitative.

Methylcodeium Sulphate.

Equivalent quantities of codeine and iodide of methyl dissolved in rectified spirit were heated in the water-bath in a sealed tube

until the iodide of methylcodeium was formed ($C_{18}H_{21}NO_3CH_3I$). It is not very soluble in water, and was decomposed by treatment with sulphate of silver, when iodide of silver was thrown down, and the easily soluble sulphate of methylcodeium was left in solution. After purification and several recrystallisations it was obtained in the form of large colourless crystals. It is an addition compound, the sulphate of methyl being simply added on to codeine.

Physiological Action of Methyl-Codeium Sulphate.

The only experiments which have been previously made with this substance are those of Crum Brown and Fraser who worked with the iodide and the sulphate. With the iodide of methyl-codeium (a tolerably soluble salt) they saw no symptoms in rabbits after 5 grains subcutaneously, while 10 and 15 grs. caused some degree of paralysis, followed by recovery in a few hours, and 20 grs. was fatal in 13 minutes. With the sulphate, 4 grains subcutaneously had no effect, while 8 grs. caused some degree of paralysis, and 10 grs. death in 19 minutes.

On frogs four experiments were performed: two of these with 2 and 2½ grs. iodide of methyl-codeium and two with 1 gr. sulphate. In all the result was death, preceded by paralysis. The paralysis commenced in from 11 to 21 minutes after administration, and in no case was the duration of the symptoms or the time of death observed. They sum up thus: "We learn from our experiments that the iodide and sulphate of methyl-codeium have a very different action from codeia. We have never observed any hypnotic effect follow their administration, and, in place of convulsions, we have seen that they produce paralysis. This, indeed, is the only marked symptom that follows their administration, and it is apparent that it does not depend on an effect on the muscles nor on the cerebral lobes." They conclude that it is due to an action on the terminations of motor nerves.

These conclusions, however, as we shall presently show, are drawn from experiments made with very large doses, doses which (especially in frogs) were much too large to show clearly any effects except those resulting from paralysis of motor nerves. Our experiments with sulphate of methyl-codeium have shown us that the addition of methyl sulphate to codeine does not alter the action of

the latter so materially as one would expect from the description of Crum Brown and Fraser.

Codeine, as we have already seen, besides acting on the brain and spinal cord, exercises a considerable depressant power on motor nerves. In the case of methyl-codeium sulphate the depressant action on the spinal cord and motor nerves is much more marked and occurs with smaller doses, while that on the cerebrum is lessened. Increase of reflexes also occurs after methyl-codeium sulphate. With small doses this increase is seen only after about 24 hours in frogs, while with larger doses it is obscured or wholly masked by the paralysis of motor nerves. If the motor nerves be protected, however, by tying the blood-vessels of a part, and the alkaloid be thrown directly into the circulation so as to reach the spinal cord at once in large amount, then we immediately get marked tetanus, just as with other members of the morphine group.

We now give an account of our experiments from which these conclusions have been drawn.

Frogs.—So small a dose as 1 milligramme subcutaneously produces well-marked symptoms of depression of the spinal cord. The motor nerves are unaffected, and next day the reflexes are slightly increased. Sometimes the increase of reflexes lasts two days.

When 2–5 milligrammes are given the same symptoms are seen.

Expt. 23.—Frog, 24 grms. R., 25 in 10 seconds.

11.0.—0·005 grm. methyl-codeium sulphate subcutaneously.

11.7.—Much less active; movements very clumsy, and jumps with difficulty. R., 17.

11.12.—Placed on back cannot recover itself; pupils small. R., 13.

11.20.—Very flaccid; responds if pinched. R., 14.

11.30.—Pupils small; corneal reflex very sluggish; responds feebly when pinched. R., 10.

11.45.—Same. Sciatic nerve exposed and stimulated at 220 mm. (Du Bois Reymond Coil) gives marked contraction of muscles.

12.30.—Quite flaccid; when pinched responds by feeble movement of legs. Sciatic nerve excitable at 220 mm.

1.0.—Is gradually recovering.

2.20.—Can now jump; no increase of reflexes, but a slight diminution still.

3.40.—Appearance and attitude normal. Reflexes are very slightly increased.

4.20.—Reflexes increased.

Next day there was still a slight increase of the reflexes.

When such a dose as 1 centigramme is given the motor nerves are much depressed in excitability but seldom completely paralysed.

Expt. 24.—Frog, 28 grms. R., 23 in 10 seconds.

12.50.—0·01 methyl-codeium sulphate subcutaneously.

1.0.—General paralysis has gradually come on. Crawls about with great difficulty. R., 9.

1.20.—Much worse. No response to severe pinching of toe. Both sciatic nerves are excitable at 190 mm. Electrodes placed over spine anteriorly (near head) cause contraction of both legs.

2.0.—Sciatics excitable at 80 mm.

3.0.—Has recovered somewhat. Both sciatics excitable at 80 mm.

4.30.—Can now jump and move about.

Next day.—Slight increase in reflexes.

After 2 centigrammes and upwards we soon get complete paralysis of motor nerves. This, however, wears off very soon, and is followed by increase of reflex excitability which sometimes amounts to tetanus.

Expt. 25.—Frog, 28 grms.

11.40.—0·04 grm. methyl-codeium sulphate subcutaneously.

12.40.—Quite flaccid. On stimulating sciatic nerves with strong current the muscles did not respond. Pupils are mere slits.

Second day.—Still very flaccid. Sciatics slightly excitable to strong current.

Third day.—Reflexes slightly increased. Sciatic nerves quite excitable.

Fourth to eighth day.—Reflexes have been greatly exaggerated.

Ninth day.—Tetanus on stimulation.

Tenth to sixteenth day.—The reflex increase has gradually passed off.

Twentieth day.—Is now quite recovered.

The amount required to paralyse motor nerves is therefore very large, the paralysis soon passes off, and as soon as it has done so the increase of reflexes can be, and is, manifested.

Small doses, or large doses given subcutaneously, always depress the cord as the first stage of their action. If, however, methyl-codeium sulphate in sufficient dose (2 centigrammes) be injected directly into the aorta (after protecting the motor nerves) we get tetanus at once without any preliminary depression of reflexes.

Five centigrammes is the usual lethal dose for a frog. This is larger than the lethal dose of codeine, but smaller amounts of it than of codeine produce physiological action. It may therefore be regarded as much more powerful than codeine (in frogs). The reason of its lethal dose being so large probably is that the paralysis of the motor nerves prevents the tetanic spasms, and thus protects the frog from the exhaustion consequent on convulsions.

Rabbits.—Crum Brown and Fraser found that methyl-codeium sulphate had no hypnotic effect on rabbits. This is hardly the case, however, although it is diminished to a very great extent.

Thus the large dose of 1 decigramme had almost no action of any kind beyond causing slight quietude.

When 0·2 grm. is given the narcotic action is well marked, and is accompanied by a certain degree of increased reflex. There seemed to be also some depression of the motor nerves which obscured the symptoms rather.

Expt. 26.—Rabbit, 1170 grms.; H., 28; R., 22 in 10 seconds.

2.45.—0·2 methyl-codeium sulphate subcutaneously.

2.55.—Has lain down.

3.0.—H., 32; R., 17. Vessels of ears very dilated.

3.5.—Pupils small, chin on table, drowsy.

3.10.—H., 29; R., 13. Ears same, pupils smaller, lies in any position in which it is placed. Slight touch of leg causes violent tetanic jerk of whole body. Quite sensitive to pain.

3.16.—Lies in any position; corneal reflex very sluggish.

3.30.—Has been lying as if deeply narcotised. Pays no attention to slight stimuli. Increased reflex has quite passed off. H., 29; R., 7. No sign of dyspnœa.

3.45.—Rabbit made a great effort and sat up. Very drowsy and shaky.

4.5.—H., 26; R., 12. Very drowsy.

Next day.—Well.

Larger doses cause death from paralysis of motor nerves. Thus

after 0·3 grm. a rabbit became drowsy, had marked increase of reflexes, and died in 55 minutes. The motor nerves were found to be paralysed.

As a result, we must conclude that methylcodeium sulphate has the same qualitative action as codeine and other members of the morphine group. Its quantitative action on different parts of the nervous system is, however, not the same.

Action of Methyl-codeium Sulphate on Rabbits.

No.	Weight of Rabbit in Grms.	Dose in Grms.	Dose per Kilo. Body Weight.	Effects.
1	1110	0·1	0·09	Very slight quietude, lasting about 2 hours.
2	1110	0·1	0·09	Quieter for about 2 hours. Respiration fell somewhat and pupils became smaller.
3	1170	0·2	0·17	Experiment in text.
4	1150	0·2	0·18	Same as Experiment 3.
5	1560	0·3	0·19	Drowsiness in 5 minutes, pupils became small, respiration much slowed, reflexes increased on stimulation, and also gave spontaneous tetanic starts. Death in 55 minutes. Heart found beating after death; sciatic nerves inexcitable.

Methocodeine.

This substance was investigated by Grimaux, who named it methocodeine, and also by Hesse, who describes it as methylmorphimethin. It is apparently the methyl ether of methylmorphine; in other words, it is codeine in which one of the non-hydroxyl atoms of hydrogen has been replaced by methyl. It is formed by the action of caustic potash on codeine methiodide.

$$C_{18}H_{21}NO_3.CH_3I + KHO = C_{19}H_{23}NO_3 + KI + H_2O.$$

We prepared the compound by the following method:—Morphine was dissolved with an equivalent quantity of soda in purified wood spirit, and an equivalent of methyl iodide added. The solution was then warmed for half an hour, allowed to cool, a second equivalent of methyl iodide and of soda added, and the solution again heated. The spirit was then evaporated, water added, and the solution exhausted with chloroform. The chloroform having

been driven off, the residue was treated with hydrochloric acid in excess, which rapidly caused crystallisation of the hydrochlorate. The crystals were pressed, and purified by recrystallisation from water and from alcohol. So obtained, the salt was nearly free from colour, and when examined under the microscope the crystals appeared to be all of the same form.

Chloroplatinate prepared in usual manner, dried in air-bath at 120° C. 0·28 grm. gave ·0522 grm. platinum, which is = 18·64 per cent.

$$(C_{19}H_{23}NO_3.HCl)_2, PtCl_4, H_2O = 18·65 \text{ per cent. Platinum.}$$

A quantity of the hydrochlorate was purified by recrystallisation, and a chloroplatinate prepared from the purified crystals.

·309 grm. (dried at 100° C.) ignited, gave ·0576 grm. Platinum, which is = 18·64 per cent.

As from these results the chloroplatinate appeared to contain a molecule of water, although dried at 120°, a portion of the compound was exposed in the air-bath at gradually increasing temperatures, but there was no distinct loss of weight till 160° was reached, at which temperature there was also distinct evidence of decomposition.

Several determinations of the chlorine were made by precipitation with nitrate of silver in the usual way.

(*a.*) ·469 of the hydrochloride (dry in water-bath) gave ·189 AgCl = ·0467 Cl, which is = 9·95 per cent.

(*b.*) ·2485 grm. gave ·099 AgCl = ·02449 grm. Cl = 9·85 per cent.

(*c.*) ·595 grm. gave ·2403 AgCl = ·05944 Cl = 9·98 per cent.

(*d.*) ·4515 grm. dry at 140°, gave ·1822 grm.; AgCl = ·0450 grm. Cl = 9·98 per cent.

Mean of the four determinations = 9·94.

$$(C_{19}H_{23}NO_3.HCl)_2.H_2O = 9·90 \text{ per cent. Cl.}$$

The alkaloid itself was obtained by dissolving the hydrochloride in water, and adding slight excess of ammonia. A viscous precipitate separated, but after standing some hours, with occasional stirring, it became a mass of small prismatic crystals. These were washed with cold water and dried in the air. The air-dry substance lost no weight in the water-bath or at 120° C. When the temperature was raised to near 135°, the substance fused and dark-

ened in colour, yet the loss of weight was only a fraction of a per cent., whence it is almost certain that the alkaloid is precipitated in the anhydrous state.

It will be observed that the chloroplatinate and the hydrochloride appear obstinately to retain water in the proportion of one molecule to two molecules of alkaloid; so that, so far as our results go, the alkaloid may have the composition $C_{38}H_{48}N_2O_7$. If that be the case, the equation above noted should be—

$$2C_{18}H_{21}NO_3.CH_3I + 2KHO = C_{38}H_{48}N_2O_7 + 2KI + H_2O.$$

Whether the water is merely combined as hydrate, or has become a constituent part of the molecule of the base, is a point we have not had opportunity of absolutely determining; but the pharmacological results are more in harmony with the latter theory.

Physiological Action of Methocodeine.

The chemical change represented in methocodeine completely alters the action characteristic of the morphine group—so much so, indeed, that points of similarity are hard to find. The distinguishing features of morphine-poisoning are wholly absent; there is no narcosis and no tetanus. The symptoms are due chiefly to poisoning of the muscles, and, to a less extent, depression of the spinal cord.

In our experiments we used the hydrochlorate, or the alkaloid dissolved in acetic acid and water.

Frogs.—A dose of 5 milligrammes usually had very little action. The animal became rather less active, but as a rule little further change was observable.

When 1 centigramme was given, however, the frog shortly became sluggish, there was depression of reflex excitability and poisoning of the voluntary muscles. The muscles at the place of injection were first affected, their electric excitability gradually becoming diminished until it completely disappeared. This condition slowly spread to all the other voluntary muscles. A similar mode of action on voluntary muscle has been observed in the case of caffeine, benzoylecgonine, and a number of other alkaloids.

The following experiment shows the usual course of poisoning after 1 centigramme:—

Expt. 27.—Frog, 27 grms. R., 21 in 10 seconds.

12.10.—0·01 grm. methocodeine hydrochloride into abdominal cavity.

12.20.—Has become much less active.

12.30.—R., 14; very shallow. Only moves if pinched.

12.50.—R. ceased. Lies on back if turned over. Draws up leg if severely pinched.

1.20—Faint response if severely pinched. Sciatic nerves excitable to very weak current.

3.20.—Frog's muscles all over body are much depressed in irritability. Many of them do not contract when stimulated with strongest electric current, others only very feebly. Motor nerves still excitable, for, when stimulated, the muscles which are not in complete rigor respond faintly. Heart stopped. Corneal reflex still present.

In many cases the poisoning of the muscles took place more slowly, death occurring only after some days, but in all the general symptoms were the same.

One centigramme proved fatal to frogs sooner or later. With larger doses also the results are the same.

Expt. 28.—Frog, 28 grms. R. in 10 seconds, 23.

12.10.—0·025 grm. under skin of abdomen.

12.25.—Has become much depressed; moves away heavily if pinched.

1.0.—Lies in any position; still responds to severe pinching.

1.40.—Sciatic nerves excitable to very weak currents (250 mm.).

2.45.—Sciatics same. Muscles of abdominal wall and left thigh are in *rigor mortis;* other muscles contract on electric stimulation.

5.30.—Muscles and sciatic nerves much diminished in electric irritability.

Next day.—Heart stopped in marked systole; none of the voluntary muscles contract to electric stimulation.

An increase of reflexes was never observed. Direct application of a solution to the exposed spinal cord or injection into the aorta depressed the reflexes or entirely abolished the vitality of the cord in a very short time.

In frogs small doses of 5 milligrammes depress the power and rate of the heart.

Rabbits.—On rabbits no narcosis was ever observed and no trace of increased reflex. The alkaloid seems to act chiefly as a muscle-poison, and to produce death by gradually poisoning the muscles. At the same time the cord is somewhat depressed. After 5 centigrammes the animal showed no symptoms except slight quietude.

When 3–5 decigrammes were given to small rabbits death occurred after a longer or shorter period from paralysis of muscles of respiration. If the alkaloid be injected at both sides of the thorax the respiratory muscles are quickly paralysed and death soon occurs, but if injected into the thighs, for instance, death is considerably delayed. The muscles in the neighbourhood of the injection first pass into a condition of *rigor mortis*, which gradually spreads to other voluntary muscles. The urine was always of a deep emerald-green colour, owing, no doubt, to some green-coloured product from dimethyl-morphine being formed in the body and excreted by the kidneys.

Apomorphine, as is well known, also tends to decompose into a green-coloured substance, and is also in a very marked degree a muscle-poison. Methocodeine, however, has not an emetic action. Even large doses had no effect on dogs beyond restlessness and anxiety. Harnack has pointed out that apomorphine, although used entirely in medicine for its emetic action, is essentially a muscle-poison, and that the emesis produced by it is, as it were, an accidental circumstance. Its chemical formula is $C_{17}H_{17}NO_2$ (that is, morphine minus H_2O), but the change is probably much more profound than the removal of a molecule of water ; and there is, no doubt, to some extent a rearrangement of the morphine molecule. Methocodeine probably resembles it in chemical constitution, just as it does in pharmacological action.

Morphine-Sulphuric Acid.

We have so named this compound, as it appears to be morphine in which a hydrogen atom is replaced by the radical HSO_3. It is described by Stolnikow as *Morphin-ätherschwefelsäure*, and we exactly followed his directions in its preparation. 20 grms. morphine and 8 grms. caustic potash were dissolved in 25 c.c. water, and to the solution was added gradually 15 grms. finely powered pyrosulphate of potash. After 12 hours the solution was diluted with 350 c.c. water, and filtered. The filtrate is rendered faintly

acid with acetic acid, whereupon the *morphin-ätherschwefelsäur.* crystallises out, while acetate of morphine and sulphate of potash remain dissolved. The precipitate is purified by crystallisation from hot water. In two successive experiments we obtained a very small yield of material. However, it was sparingly soluble, did not give the morphine colour-reactions, and gave a considerable precipitate when warmed with barium chloride and hydrochloric acid, so that we have no doubt the substance was practically the same thing as obtained by Stolnikow. According to that writer, the reactions which take place in the formation of the compound are as follows:—

$$C_{17}H_{18}NO_2HO + KHO = C_{17}H_{18}NO_2KO + H_2O .$$

$$C_{17}H_{18}NO_2KO + K_2S_2O_7 = C_{17}H_{18}NO_2.KO.SO_3 + K_2SO_4 .$$

$$C_{17}H_{18}NO_2.KO.O.SO_2 + C_2H_4O_2 = C_{17}H_{18}NO_2.O.SO_2.OH + C_2H_3KO^2 .$$

Physiological Action of Morphine-sulphuric Acid.

The only experiments on the action of this substance are those of Stolnikow. He attempts to prove that the characteristic action of morphine depends on the phenol-like hydroxyl group which it contains, and contends that when the H. of this hydroxyl is substituted by an alkyl radical another physiologically active element is introduced into the molecule. But if a physiologically inactive substance (such as he assumes HSO_3 to be) be introduced, then the action is very materially altered.

He found in frogs that doses of 5 milligrammes of his compound were almost inert, but that after 3 to 5 times this dose tetanus occurred almost at once. He ranks it among the tetanising group of opium alkaloids. From his experiments Stolnikow concludes that with the hydroxyl group (OH) in morphine is bound up—(1) its narcotic properties, its property of acting specially and chiefly on the cerebrum; (2) its poisonous properties.

Although our experiments confirm Stolnikow's to a certain extent, viz., that the activity of morphine is diminished by the substitution of HSO_3 for H., yet it is not diminished to anything like the extent which he states, nor is the hydroxyl group so important a constituent as he makes out. The new body retains the morphine action—a narcotic and tetanic stage—perfectly, although its toxicity is less

than that of morphine. The substance is practically insoluble in water and dilute acids, and for our experiments had to be dissolved in a small amount caustic soda.

Frogs.—One centigramme caused lethargy, followed by an increase in reflexes. When the dose was increased to 2 centigrammes the two stages became very distinct, and tetanus was developed.

Expt. 29.—Frog, 33 grms. R., 22 in 10 seconds.

11.38.—0·02 subcutaneously in solution.

11.40.—Heavy and sluggish.

11.46.—Corneal reflex gone. Will not lie on back.

11.52.—Very heavy and sluggish; jumps very abortive. R., 15. Reflexes are greatly diminished.

12.0.—R. has ceased. Lies in any position.

12.30.—Reflexes increased.

12.40.—Gives start on stimulation.

2.30.—Almost tetanus on stimulation.

4.0.—Same.

Next day.—Tetanus on stimulation.

Larger doses were fatal; the narcotic stage is not very deep and tetanus soon supervenes, followed by exhaustion.

Rabbits.—As we had only a small quantity of the substance, we made only four experiments on rabbits. The result showed that its narcotic power in rabbits is very much less than that of morphine. We had not sufficient to test its tetanising power.

The experiments are given in the following table:—

Action of Morphine Sulphuric Acid Ether on Rabbits.

No.	Weight of Rabbit in Grms.	Dose in Grms.	Dose per Kilo. Body Weight.	Effects.
1	1000	0·02	0·02	Scarcely any effect. Was a little quieter for about 2 hours, during which respiration fell from 19 to 12 per 10 seconds.
2	1000	0·04	0·04	Was distinctly lethargic for 3 hours, but hardly amounting to narcosis.
3	1412	0·05	0·035	Same as Experiment 2.
4	1412	0·1	0·07	Was much quieter for 3 hours. Respiration fell from 16 to 9 per 10 seconds. Narcotic effect very slight.

Chlorocodide.

We first tried to prepare this compound by the action of strong hydrochloric acid on codeine, after the manner described by Wright. The solution of the hydrochloride was precipitated in fractions, but each of the fractions gave a chloroplatinate indicating a different molecular weight; whence it was concluded that the substance was a mixture, a fact which was confirmed by the physiological experiments. We then tried the method of Gerichten. 13·5 grms. of codeine hydrate was dried at 120° and mixed with 10 grms. of pentachloride of phosphorus; 50 grms. of oxychloride of phosphorus was then poured over the mixture, whereon a violent action ensued. After some hours the clear solution was diluted, ammonia added, and the precipitate received on a filter. The precipitate was washed, pressed, and dried by exposure to the air; then dissolved in chloroform, which was left to evaporate. On adding alcohol to the residue, it became filled with crystals, which were purified by pressure and recrystallisation. The substance so obtained yielded a chloroplatinate, which after drying in exsiccator lost no further weight at 100°.

·3195 grm. gave ·060 grm. Pt., which is = 18·77 per cent.

$$(C_{18}H_{20}ClNO_2.HCl)_2.PtCl_4 = 18{\cdot}73 \text{ per cent. Pt.}$$

Physiological Action of Chlorocodide.

Gee has previously made a few experiments with this substance. He gave 9½ grains in divided doses to a cat, which ultimately died in convulsions, the chief symptoms being salivation, dilatation of the pupils, restlessness, and a mixture of tetanus and paralysis. Consciousness was unaffected. In Gee's opinion, the action is exactly like that of codeine. A dose of ¼ grain per os, and subcutaneously in man, had no effect.

For our experiments we used a neutral solution in acetic acid and water.

Frogs.—One milligramme caused in frogs sluggishness and depression of reflexes, followed by an increase of reflexes. With larger doses the exhaustion (or depression) accompanying or following the tetanus was extremely marked at first, but gradually passed

off, leaving only tetanus. The same thing has been noted as occurring with morphine.

The following experiment will serve as an example :—

Expt. 30.—Frog, 27 grms.

1.50.—0·005 chlorocodide subcutaneously.

2.0.—Pupils very small; respiration ceased; reflexes dull. Lies on back if turned over, struggles if pinched.

2.7.—Spinal reflexes now a good deal increased; no corneal reflexes.

2.10.—Least touch causes violent jerk, but frog lies quite exhausted and flaccid after it. Pupils are mere slits.

3.30—Same.

5.30.—Gives faint tetanic jerk when stimulated.

Second day.—Reflexes greatly increased.

Third day.—Greater increase reflexes.

Fourth day.—In violent tetanus.

When a larger dose, such as 1 centigramme, was given, there was great depression of the cord, so much so that only a faint indication of the tendency to tetanus was observable in many cases, although in others it was well marked. One centigramme by the mouth caused well-marked but not extreme depression, followed shortly by tetanus.

Two centigrammes injected into the aorta produced tetanus at once.

The paralysing effect of chlorocodide on motor nerves is about equal to that of codeine; 15 milligrammes injected into the iliac artery destroyed the electric excitability of the sciatic nerve in a few minutes.

So far the action of chlorocodide closely resembles that of codeine, but is more depressing to the spinal cord. In addition, however, it is a very decided muscle-poison. When injected subcutaneously the muscles in the immediate neighbourhood very soon become poisoned and cease to respond to electric stimuli, while those at a distance seem to be little affected.

The heart also is very much depressed.

In frogs 1 centigramme sooner or later proves fatal.

Rabbits.—In rabbits doses of 2–5 centigrammes caused marked weakness in legs, depression of the spinal cord, slow and weaker

action of the heart, and a great inclination to lie down. There was no narcosis.

When larger doses, up to 1 decigramme, are given, the depression of the spinal cord is extreme, but there is also a tendency to tetanic jerking. The symptoms are exactly as if a spinal stimulant and depressant had been given together.

When still larger doses are given tetanus of a very decided character is seen, but towards the end of the poisoning becomes very feeble owing to the marked weakness of the whole muscular system.

The following experiments show the action of large doses:—

Expt. 31.—Rabbit, 1800 grms.

12.5— 0·15 grm. chlorocodide subcutaneously.

12.12.—Legs weak, gait affected.

12.14.—Cannot walk; occasionally gives slight start.

12.16.—Tetanic attack with opisthotonos.

12.18.—Clonic spasms.

12.35.—Has had frequent clonic spasms not amounting to tetanus.

1.10.—Increased reflex passing off; legs very weak; has to lie down.

3.0.—Is recovering; can drag itself about.

4.20.—Nearly well.

Expt. 32.—Rabbit, 1760 grms.

11.44.—0·2 grm. chlorocodide subcutaneously.

11.48.—Slight tetanus; can still walk.

11.54.—Severe tetanus, succeeded by very great flaccidity and depression.

11.59.—Lying flaccid, and evidently suffering from muscular weakness. Occasionally gives a slight start.

12.15.—Same.

12.40.—Has frequent clonus of head; the rest of body less affected.

12.55.—Tendency to spasm much greater. The jerkings are almost continuous. Had one severe tetanic spasm with opisthotonos.

1.55.—Has been in almost continuous clonic spasms.

2.50.—Died quietly.

Post-mortem immediately.—Heart stopped; not excitable to electric stimulation; peristalsis good; the voluntary muscles all over the

body contract very slightly to strong electric currents, and even this feeble amount of contraction disappeared in about five minutes.

Chlorocodide therefore partakes partly of the action of codeine and partly of the action of apomorphine.

Trichloromorphide.

There do not appear to be any published results giving experiments on the action of phosphorus pentachloride or oxychloride on morphine. When anhydrous morphine and phosphorus pentachloride are mixed and heated, there is evidently some action; but we could not obtain in this way any considerable amount of material other than morphine, or possibly one of the polymerised morphine bodies described by Wright. It was impossible to try any experiment on a solution of morphine, as there is no known solvent for that alkaloid which is not at once attacked by the pentachloride. We therefore adopted the method used by Gerichten for the preparation of chlorocodide. 13 grms. anhydrous morphine and 17 grms. phosphorus pentachloride were mixed, and several grms. of oxychloride of phosphorus poured over the mixture. The mixture was left for some hours, with occasional agitation, until solution was complete. It was then diluted with water, and ammonia added. The precipitate so obtained was slightly washed, dried, and treated with chloroform. On evaporation the chloroform left a non-crystalline residue, from which a platinum salt was prepared. Of this compound ·091 grm. gave 0·16 grm. Pt. = 17·58 per cent. The portion of the precipitate undissolved by the chloroform was treated with boiling alcohol, which on evaporation left a partially crystalline coloured residue. This yielded a chloroplatinate of which ·1575 grm. gave ·0275 grm. Pt = 17·45 per cent.

$$(C_{17}H_{16}Cl_3NO)_2 \cdot Pt\,H_2\,Cl_6 = 17{\cdot}43 \text{ per cent. Pt.}$$

It would be too much to assume that that is certainly the composition of the substance obtained, as it is quite conceivably a mixture. We anticipated that the two hydroxyls would be replaced by chlorine atoms, but the chlorination seems to have gone further. As from the formation of methocodeine, there appears to be in the morphine molecule a non-hydroxyl hydrogen atom which is more readily replaced than the others, it has possibly in the compound

under consideration been replaced by chlorine. However that may be, we regard the physiological results obtained with this substance as of considerable value, as they indicate the general modification in the action of morphine caused by the substitution of chlorine for the hydroxyl groups.

Physiological Action of Trichloromorphide.

Frogs.—Trichloromorphide acts primarily on the central nervous system, causing depression of the spinal cord, followed by tetanus. It has, however, even in moderate doses, a marked paralysing effect on motor nerves, which tends to obscure the tetanus while it lasts. The paralysis is, however, somewhat slow in appearing, and soon wears off, usually leaving the animal in violent spasm. In addition, trichloromorphide has some slight action as a muscle-poison.

The following experiments show its action in all these respects:—

Expt. 33.—Frog, 30 grms.

12.8—0·005. grm. trichloromorphide subcutaneously.

12.12.—Reflexes slightly diminished.

12.15.—Reflexes very distinctly depressed, but frog still moving about.

12.20.—Sluggish; lies on back; pupils small. Reflexes slightly increased.

12.25.—Slightest touch causes violent start.

12.30.—Spontaneous tetanus, followed by great flaccidity. Slightest touch brings on tetanus.

12.37.—Now gives a mere twitch on stimulation.

12.42.—No response on stimulation. Sciatic nerves inexcitable to strong electric current.

Second day.—In violent tetanus.

Third to fifth day.—In tetanus.

Sixth day.—Dead.

If the sciatic nerve of one leg be protected by tying the femoral artery, and 5 milligrms. be given, the protected nerve is not paralysed while all the others are.

Expt. 34.—Frog, 30 grms.

12.46—0·01 grm. trichloromorphide subcutaneously under skin of back.

12.58.—Has gradually become deeply paralysed.

1.10.—Slight increase in reflexes.

1.20.—Reflexes greatly increased.

1.45.—Motor nerves much depressed in excitability. Reflex increase can scarcely be manifested.

2.15.—Motor nerves paralysed.

3.0.—Muscles of back paralysed and not excitable to interrupted current; other muscles of body excitable; heart still beating.

Rabbits.—In rabbits 1–4 centigrammes caused slight narcosis, and very slight tendency to increased reflexes. Respiration diminished greatly in frequency, and the heart slightly.

Doses of from 8–25 centigrammes caused varying degrees of increased reflex without marked narcosis. The larger the dose the less tendency there was to narcosis, but the more to paralysis of motor nerves with consequent paresis and depression.

Expt. 35.—Rabbit, 1635 grms. H., 32; R., 22 in 10 seconds.

11.50.—0·25 trichloromorphide subcutaneously.

11.56.—Has lain down. R., 7; H., 31. Ears very hyperæmic.

12.0.—Legs very weak; slight increase in reflexes. H., 30; R., 7. Pupils larger.

12.5.—Reflexes markedly increased, but there is also very considerable paralysis. Pupils widely dilated.

12.12.—Slight tetanic attack.

12.35.—Lying on belly; every few seconds gives a spasmodic twitch. R., 7.

2.50.—Has been much in same condition. There has been no narcosis.

5.0.—The increase of reflexes is wearing off, but there is still great depression.

7.0.—Increased reflex gone.

8.0.—Still great depression.

Next day was quite well.

In this experiment, as in others with large doses, it was evident that the partial paralysis of the motor nerves prevents the full manifestation of the tetanus, and probably thereby prevents death from convulsions.

The action of trichloromorphide resembles qualitatively that of the other morphine derivatives; its action on motor nerves is markedly paralysing, and it is in addition a not very powerful muscle poison.

Nitrosomorphine.—$C_{27}H_{18}(NO)NO_3.H_2O$.

This compound was prepared by E. L. Mayer by passing nitrous fumes (prepared by action of nitric acid on arsenious acid) into a solution of morphine. The composition indicated is the same as that of morphine nitrite, and there seems to be some little doubt as to the purity of the substance.

Physiological Action of Nitrosomorphine.

Nitrosomorphine is a very insoluble substance, and we had difficulty in getting a proper solution for subcutaneous injection in frogs. We therefore gave it by the mouth in substance. When 0·015 grm. was so given, the animal gradually developed a narcotic condition, followed in due time by increase of reflexes and tetanus.

In rabbits 4 or 5 centigrammes subcutaneously caused light narcosis, lasting four or five hours. We found it difficult to give larger doses.

These experiments, although not very satisfactory, show that nitrosomorphine has the same qualitative effect as morphine.

Physiological Action of some other Morphine Derivatives.

C. R. A. Wright prepared a number of morphine derivatives which were examined by M. Foster and by R. Stocker.

Deoxymorphia.—$\left(C_{17}H_{19}NO_2(C_{17}H_{17}NO\begin{matrix}H\\OH\end{matrix}\right)$.

Deoycodeia.—$C_{18}H_{21},NO_2$.

With these substances Foster found that 5 centigrammes caused in cats excitement, while 1 decigramme caused convulsions and excitement.

Bromotetramorphia.—$C_{68}H_{75}BrN_4O_{12}$ (4 molecules of morphine coalesced together and one H. replaced by Br.).

Bromotetracodeia.—$C_{72}H_{83}BrN_4O_{12}$.

Chlorotetracodeia.—$C_{72}H_{83}ClN_4O_{12}$.

These all acted in same way. The hydrochlorates were used.

In cats 1 decigramme caused excitement and symptoms similar to morphine. In rabbits the same amount caused only slight excitement.

Stocker experimented with *dicodeia* (2 molecules of codeine

coalesced together), *tricodeia* and *tetracodeia*, on cats and dogs. These all had a very similar action to codeine.

The substances $C_{68}H_{81}IN_4O_{10}.4HI$ from codeine and $C_{68}H_{82}I_2N_4O_{10}.4HI$ from codeine and morphine in doses of 1–3 decigrammes had little effect beyond causing looseness of the bowels in dogs.

The experiments are not sufficient to enable us to determine the action of these substances beyond saying that they have a generic resemblance to morphine and codeine.

Oxydimorphin.—$C_{34}H_{30}N_2O_6$ (2 molecules of morphine minus 2H). This is the oxymorphine ($C_{17}H_{19}NO_4$) of Schützenberger.

According to Diedrich this substance has no narcotic action. It weakens the heart and causes diarrhœa and vomiting. The emesis occurs after subcutaneous injection, and, according to Diedrich, is due to an action on the medulla. Its frequent intravenous administration caused changes in the gastric mucosa, hyperæmia, swelling, and ulceration. Probably, therefore, it is excreted from the blood into the stomach and bowel, and by its local irritation acts as an emetic and purgative reflexly through the vagus.

Nothing is known as to its constitution, but it is probably allied to apomorphine more closely than to morphine.

Marmé and Diedrich express the opinion that morphine is converted into oxydimorphine in the blood, and hence its frequent nauseating and purgative effects when given subcutaneously. It is more probable, however, that the excretion of morphine into the stomach (Alt) causes these symptoms independently of any chemical change.

Summary.

I. The methyl (codeine), ethyl (codethyline), and amyl ethers of morphine form a group of substances having exactly similar actions. In all the same hydrogen atom has been replaced in morphine by an alkyl radical; they are therefore substitution derivatives. It seems to be a matter of indifference which radical is introduced, so long as it replaces the same hydrogen atom in morphine. In all the narcotic action of morphine is much diminished, the tetanising action and the paralysing action on motor nerves are increased, while the lethal dose (on account of the greater tendency to convulsions) is much smaller.

The action is, however, of essentially the same nature as the morphine action; the same parts of the central nervous system are affected, and in the same way as by morphine, but not in the same degree.

This is what one would expect from chemical considerations, for in making these substances no profound change has been effected in the morphine molecule, but simply an alkyl radical has been introduced into one of the outlying groups which compose it.

II. Acetyl-, diacetyl-, benzoyl-, and dibenzoyl-morphine form a group of substances having exactly similar actions. In them one or both of the hydroxyl hydrogens of morphine have been replaced by an acid radical.

Comparing them with morphine, their action is the same in kind but differs in degree. Their tetanising power is much greater, while their narcotic action, although visible after smaller doses, is not nearly so profound. Increase of dose, instead of deepening the narcosis, brings on tetanus.

Comparing them with codeine, they induce an equal narcotic effect (rabbits) with about one-tenth of the dose, while a dose about three times larger is necessary to induce tetanus. Their depressing action on motor nerves is about the same.

It seems quite indifferent which radical is introduced, and whether one or both of the hydroxyl hydrogens are replaced. Just as with codeine and its analogues, no great change has been made in the morphine molecule, but simply in the outlying hydroxyl groups.

III. In morphine-sulphuric acid and nitroso-morphine the radicals HSO_3 and NO replace the hydroxyl hydrogen atoms, and the action is modified much in the same way as by the introduction of other acid radicals (II.).

IV. Chlorocodide and Trichloromorphide are chlorine derivatives; in the former Cl replaces the OH of codeine, while in the latter both hydroxyl groups, and in addition one H atom of morphine, have been replaced by 3 Cl.

They retain the characteristic actions of morphine on the nervous system, but are in addition marked muscle poisons.

V. In metho-codeine two methyl molecules have been introduced into morphine, one of which replaces a hydroxyl hydrogen atom, while the other replaces an H in the body of the morphine molecule.

This completely alters the character of the action, as metho-codeine has the action of a muscle poison (like apomorphine).

VI. Methylmorphium chloride and methylcodeium sulphate are addition products, formed by adding on methyl chloride and methyl sulphate respectively to the intact morphine or codeine molecule. The action is not profoundly altered by the chemical change.

The paralysing action on motor nerves is considerably increased, and the narcotic action is lessened, but qualitatively the effects on the animal organism remain similar to those of morphine or codeine.

The chemical change made in the intimate structure of the morphine molecule has not been profound; there has been simply the addition of a radical, and hence one would scarcely expect the action to be much altered.

VII. Other morphine and codeine derivatives which have been examined by other investigators seem to retain essentially the morphine action.

With regard to morphine, it seems certain that so long as the chemical changes are restricted to what may be called the outlying groups of the molecule, very little alteration takes place in the physiological action.

The change which does take place does not depend so much on the substituting body, as on what part of the molecule is substituted.

When a change is made in the kernel or groundwork of the molecule, then the action is much more profoundly altered.

Literature of Morphine and its Derivatives.

We do not include in this list papers which are of purely toxicological or therapeutical interest. Certain writers have already published a considerable portion of the morphine literature, and as their papers are easily accessible to scientific workers we do not enumerate here the papers which they mention.

Alt, *Berlin. klin. Wochenschr.*, p. 560, 1889, "Untersuchungen über die Ausscheidung des subcutan-injecirten Morphin durch den Magen."

Binz, *Archiv f. expt. Path. and Pharmak.*, vi. 310, 1877, "Zur Wirkungsweise schlafmachender Stoffe." *Deutsche Med. Wochenschr.*, pp. 615, 627, 1879; p. 149, 1880, "Ueber den arteriellen Druck bei Morphiumvergiftung."

Bochefontaine, *Comptes Rendus*, xciii. 743, 1881, "Sur l'action physiologique de la codéthyline."

Bouchut, *Comptes Rendus*, lxxiv. 1289, 1872, "Recherches sur l'action des bases tirées de l'opium, &c."

Brown and Fraser, *Trans. Roy. Soc. Edin.*, xxv. 1, 1868–9, "On the Connection between Chemical Constitution and Physiological Action."

Brunton and Cash, *Cbl. f. d. med. Wiss.*, p. 241, 1886, "Temperaturerniedrigende Wirkung des Morphin auf Tauben.

Calvet, *Essai sur le morphinisme aigu*, Paris, 1877.

Camus, *Gazette Hebdomadaire*, p. 498, 1865, "Recherches expér. sur l'antagonisme de l'opium et de la belladonne."

Commission Brit. Med. Assoc., *Brit. Med. Jour.*, ii. 486, 1874, "On the Antagonism of Medicines."

Deguise, Dupuy, and Leuret, *Recherches et Expériences sur les effets de l'Acétate de Morphine*, Paris, 1824.

Diedrich, *Ueber Oxydimorphin*, Inaug Diss., Göttingen, 1883.

Dietl and Vintschgau, *Pflüger's Archiv*, xvi. 316, 1878, "Das Verhalten der physiologischen Reactionszeit unter dem Einfluss von Morphium, Caffein, und Wein.

Eckhard, *Beiträge zur Anat. and Physiol.*, viii. 77, 1879, "Ueber den Morphium-diabetes" (gives literature of morphine diabetes).

Falck, *Toxicologische Studien ueber das Hydrocotarnin*, Inaug. Diss., Marburg, 1872.

Filehne, *Archiv f. expt. Path. and Pharmak.*, x. 442, xi. 45, 1880, "Ueber die Einwirkung des Morphins auf die Athmung," *Berlin klin. Wochenschr.*, 1887.

Foster, *Proc. Roy. Soc.*, xix. 1870–1.

Fröhlich, *Rossbach's Pharmak. Untersuchungen*, i. 186, 1873, "Beiträge zur Lehre von dem physiol. Antagonismus in der Wirkung der Gifte."

Fronmüller, *Klinische Studien der narkotischen Arzneimittel*, Erlangen, 1869.

Fubini, *Cbl. f. die med. Wiss.*, p. 773, 1880, "Ueber den Einfluss der wichtigsten Opium-alcaloide auf die Menge des von Menschen in 24 Stunden ausgeschiedenen Harnstoffs." *Ibid.*, p. 579, 1882, "Einfluss der elec. Inductions-ströme, &c., auf die Geschwindigkeit der Bewegungen des Dünndarms."

Gee, *St Bartholomew's Hosp. Reports*, v. 215, 1869, "Note upon Apomorphia and Chlorocodide."

Grasset and Amblard, *Comptes Rendus*, xciii. 373, 1881, "De l'action convulsivante de la Morphine chez les Mammifères."

Heubach, *Archiv f. expt. Path.*, viii. 31, 1878, "Antagonismus zwischen Morphin und Atropin."

Krage, *Ueber Albuminurie and Glycosurie nach Morphium*, Inaug. Diss., Greifswald, 1878.

Landsberg, *Pflüger's Archiv*, xxiii. 413, 1880, "Untersuchungen über das Schicksal des Morphins im lebenden Organismus."

Legg, *St Barts. Hosp. Reports*, vi. 97, 1870, "Observations on the physiol. Action of Apocodeia, &c."

Lenhartz, *Archiv f. expt. Path. and Pharmak.*, xxii. 335, 1887, "Exper. Beiträge zur Kenntniss der acuten Morphinvergiftung und des Antagonismus zwischen Morphin and Atropin."

LEVINSTEIN, *Die Morphiumsucht*, Berlin, 1877. Translated by Chas. Harrer, London, 1878.

MARMÉ, *Deut. Med. Wochenschr.*, p. 197, 1883, "Untersuchungen zur acuten and chromischen Morphinvergiftung" (M. found in urine and fæces).

MEIHUIZEN, *Pflüger's Archiv*, vii. 201, 1873, "Ueber den Einfluss einiger Substanzen auf die Reflexerregbarkeit des Rückenmarks."

NASSE, *Beiträge zur Physiologie der Darmbewegung*, Leipzig, 1866.

NOTHNAGEL, *Virchow's Archiv*, lxxxix. 1, 1882, "Ueber die Einwirkung des Morphin auf den Darm."

OTT, *Journ. of Mental and Nervous Diseases*, Chicago, 1878, "Action of the Alkaloids of Opium."

PICARD, *Comptes Rend.*, lxxxvi. 1144, 1878, "Sur l'action de la morphine chez les chiens."

PIERCE, *Practitioner*, ii. 437, 1875, "On the Phys. Action of some new Morphine and Codeine Derivatives."

PREISENDÖRFER, *Deutsche. Archivs f. klin. Med.*, xxv. 40, 1880, "Zur Lehre von der Wirkung der Narkotika."

RABUTEAU, *Comptes Rend.*, lxxiv. 1109, 1872, "Recherches sur les propriétés de divers principes immédiats de l'opium."

RABUTEAU ET PAPILLON, *Comptes Rend.*, lxxvii. 1370, 1873, "Observations touchant l'action de certaines substances toxiques sur les poissons de mer."

SCHROEDER, *Archiv f. expt. Path.*, xvii. 96, 1883, "Untersuchungen über die pharmakologische Gruppe des Morphins."

STOCKER, *Proc. Roy. Soc.*, xx. 1871–2.

STOLNIKOW, *Ztschr. f. phys. Chemie*, viii. 235, 1884, "Ueber die Bedeutung der Hydroxylgruppe in einigen Giften" (gives literature regarding excretion of morphine in urine).

TAUBER, *Archiv f. expt. Path.*, xxvii. 336, 1890, "Ueber das Schicksal des Morphins in theirischen Organismus" (literature).

VULPIAN, *Comptes Rend.*, xciv. 555, 1882, "De l'action qu'exercent des fortes doses de Strychnine sur la motricité des nerfs chez les mammifères.

WACH, *Beitrag zur Path. und Ther. der chronischen Morphiumvergiftung*, Inaug. Diss., Jena, 1880.

WITKOWSKI, *Archiv f. expt. Path.*, vii. 247, 1877 (gives the pharmacological literature very fully—35 papers).

HUSEMANN AND HILGER, *Die Pflanzenstoffe*, 2 ed., vol. ii., 1884 (gives the chemical literature very fully, and also a few papers not mentioned by Witkowski or by us).

Literature of Codeine.

WACHS, *Das Codein*, Inaug Diss., Marburg, 1868 (literature from 1832–68).

SCHROEDER, *Archiv expt. Path.*, xvii., 1883 (gives literature partially).

CL. BERNARD, *Les Anesthésiques et l'Asphyxie*, 1875.

BAXT, *Reichert and Du Bois Reymond Archiv*, p. 112, 1869, "Die physiol. Wirkung einiger Opium-Alkaloide."

WEIR MITCHELL, *Amer. Jour. of Med. Sc.*, lix., 1870, "On the Effects of Opium and its Derivative Alkaloids."

HARLEY, *Old Vegetable Neurotics*, London, 1869.

Among the authors previously cited Bouchut, Crum Brown and Fraser, Falck, Fronmüller, Ott, and Rabuteau describe the action of Codeine.

A New Method for Determining Phosphorus in Organic Phosphorus Compounds. By Prof. E. A. Letts and R. F. Blake, Esq., *Queen's College, Belfast.*

Read July 21, 1889.

In the phosphines and their derivatives, which we have investigated from time to time, considerable uncertainty has always attended the determinations of phosphorus by the ordinary methods recommended for the purpose. In fact, we never felt any confidence in the result, for no matter how carefully the determinations were made, duplicate analyses led to different numbers.

The uncertainty depends partly upon the difficulty of oxidising the phosphorus in such compounds to phosphoric acid. For, as a rule, in any *dry* combustion process which may be employed, volatile oxidation products, containing phosphorus, are formed of great stability, which frequently pass over the red-hot oxidising mixture almost unchanged. Moreover, the glass of the tube is attacked by the oxidising mixture, and this undoubtedly leads to inaccuracies, probably of considerable magnitude.

If any *moist* combustion process is resorted to, only a part of the phosphorus is converted into phosphoric acid. A more certain and trustworthy process than any of those which are in general use was therefore desirable, and in some of our investigations, where everything depended upon a correct estimation of the phosphorus, it was essential. It occurred to one of us that the difficulty in finding such a process ought not to be so great after all, for by burning a substance in the ordinary way with oxide of copper, the phosphorus ought to be completely oxidised, and should be found at the end of the operation as phosphate of copper, in which it could be estimated without much difficulty by the molybdate method. In addition to the simplicity of such a method, it should also possess the great advantage of permitting the simultaneous determination of carbon and hydrogen.

Our anticipations have been fully realised, and the new process, based on the above principle, if somewhat tedious, we believe to be accurate, of general application, and easily carried out. We shall describe it with the necessary detail, and then give some of our results obtained with it.

At the outset we experienced some difficulty in obtaining pure oxide of copper; for any commercial samples we examined, even when called "pure" by the manufacturer, were invariably found to contain phosphorus or arsenic, often in considerable quantity. We therefore prepared it for ourselves, as follows:—

Commercial sulphate of copper was dissolved in water, and the solution saturated with chlorine. On evaporation and crystallisation, fairly pure crystals were obtained, but we preferred to recrystallise them once. The cold saturated solution of this salt was then electrolysed in a large beaker or shallow glass dish, the electrodes being platinum plates, of the size generally used in the cell of a Grove's battery. The gas engine used to drive our dynamo was 1½ horse-power, and with it we obtained about 100 grms. of pure copper in 7–8 hours. The copper was then dissolved in pure nitric acid in a platinum dish, the solution evaporated, and the residual nitrate of copper calcined at a dull red heat with constant stirring. The resulting oxide formed a black crystalline powder.

In making the phosphorus determination, a narrow tube about 50 cm. long and 7–8 mm. bore was employed, which was found to contain, when properly charged, about 25 grms. of oxide of copper. It was drawn out in the usual way, and sealed at the posterior end; then charged first with about 5 cm. oxide of copper, afterwards with about 10–15 cm. of the mixture of substance and oxide, and finally with the pure oxide, the whole being kept in its place by a plug of copper gauze. The combustion was then proceeded with in the ordinary manner, and a current of oxygen passed at its conclusion.

When cold, the tube was removed from the furnace, the plug of copper gauze hooked out, and the remainder of the contents shaken into a beaker. The tube was then washed two or three times with hot nitric acid, to dissolve every trace of adhering oxide of copper. To do this, the broken (tail) end was sealed up again, the tube filled with nitric acid, and heated in front of an ordinary fire. The nitric acid washings were added to the contents of the tube, which had been shaken into the beaker, and a further quantity of the same acid added—sufficient to dissolve the whole of the oxide of copper, for which about 100 c.c. in all were usually required. Solution of the oxide was effected by boiling. When all had dissolved, about 200

c.c. of the molybdate of ammonia mixture* was added, and the whole placed in a bath with a thermo-regulator at 50° C. for about twenty-four hours. The precipitate was then collected, and the phosphorus converted in the usual way into pyrophosphate of magnesium.

In our first experiments we aimed only at determining the phosphorus, while later we determined carbon and hydrogen in addition.

The following are the chief results we have as yet obtained:—

Determinations of Phosphorus only.

1. In oxide of tribenzyl phosphine—

(A) 0·4477 gave 0·1582 $Mg_2P_2O_7$ = 0·04418 P = 9·86 per cent.
(B) 0·3294 „ 0·1178 „ = 0·032899 P = 9·98 „

	Obtained. A	Obtained. B	Calculated for $(C_7H_7)_3PO$
Phosphorus,	9·86	9·98	9·68

2. In dibenzyl-phosphinate of methyl—

0·2592 gave 0·1088 $Mg_2P_2O_7$ = 0·0304 P = 11·72 per cent.

	Obtained.	Calculated for $C_{15}H_{17}PO_2$
Phosphorus,	11·72	11·92

3. In tribenzyl phosphine—

0·3634 gave 0·1352 $Mg_2P_2O_7$ = 0·037758 P = 10·39 per cent.

	Obtained.	Calculated for $(C_7H_7)_3P$
Phosphorus,	10·39	10·19

4. In mono-benzyl phosphinic acid—

0·2559 gave 0·1608 $Mg_2P_2O_7$ = 0·044908 P = 17·54 per cent.

	Obtained.	Calculated for $(C_7H_7)H_2PO_3$
Phosphorus,	17·54	18·02

Determination of Phosphorus, Carbon, and Hydrogen.

1. In dibenzyl phosphinic acid—

$$0{\cdot}5639 \text{ gave } \begin{cases} 1{\cdot}4058\ CO_2 = 0{\cdot}3834\ C = 67{\cdot}99 \text{ per cent.} \\ 0{\cdot}3315\ H_2O = 0{\cdot}036833\ H = 6{\cdot}53 \text{ per cent.} \\ 0{\cdot}2639\ Mg_2P_2O_7 = 0{\cdot}073701\ P = 13{\cdot}06 \text{ per cent.} \end{cases}$$

	Obtained.	Calculated for $(C_7H_7)_2HPO_2$
Carbon,	67·99	68·29
Hydrogen,	6·53	6·09
Phosphorus,	13·06	12·60

These examples show that even when small quantities of the phosphorised compounds are submitted to analysis, fairly accurate results are obtained—far more accurate, indeed, than might have been anticipated, considering the very large quantity of nitrate of copper with which the phosphoric acid was diluted previous to precipitation by the molybdate. It is probable that with larger quantities even better numbers would have resulted.

* Prepared according to Frsenius' *Manual.*

List of the Fossil Dipnoi and Ganoidei of Fife and the Lothians. By R. H. Traquair, M.D., F.R.S.

(Read July 21, 1890.)

No district of Great Britain is so rich in genera and species of Carboniferous Dipnoi and Ganoidei as that of Fife and the Lothians, and in the frequency of entire specimens of these fishes it is only approached by North Staffordshire.

Another feature of the greatest geological interest is the abundance of fish remains in estuarine strata below the horizon of the Millstone grit, whereby a means of comparing the Ganoid fish faunæ of the Upper and Lower divisions of the Carboniferous formation is afforded. Lower Carboniferous Ganoids and Dipnoans occur, it is true, in the western part of the great central Scottish carboniferous area, and a very remarkable assemblage of fishes of that period was discovered not many years ago in the district of Eskdale and Liddesdale in the south. But in England and Ireland and in other parts of the world generally, the Lower Carboniferous fish remains known to us are mainly those of marine Selachii, so that an especial interest attaches to the Carboniferous fish-fauna of Scotland, and particularly to that of the district with which the present list has to deal.

A list of the fossil fishes of the Edinburgh district, compiled by the late Mr Salter, is given in the *Memoir of the Geological Survey*, explanatory of Sheet 32 (Scotland), and published in 1861. Since the appearance of this Memoir, now nearly thirty years ago, no collective list of the fossil fishes of this part of the country has been drawn up, though the amount of material amassed by the labours of many collectors has enormously increased during those years. Among those who have contributed, by their work in the field, to an augmentation of the material for a new and expanded list of the fossil fishes of this district, I may mention, besides Messrs Bennie and Macconochie, collectors to the Geological Survey of Scotland, Mr W. Tait Kinnear, Mr W. Anderson, now of the Geological Survey of New South Wales, Mr T. Stock, Mr James Kirkby, and the late Mr Robert Walker of St Andrews; while I myself, during the past sixteen years, have been able to procure both for

the Museum of Science and Art and for my own collection, a very considerable number of specimens from the localities round Edinburgh.

One result of this large increase of material is that a number of undoubtedly new species have to be chronicled. Another result is, that the increase of knowledge which this increase of material has brought with it, induces me to withdraw several species which I myself had previously named, but which I now must place among the synonyms. Where species have been otherwise rectified, I have not considered it necessary, in a list like this, to occupy space by giving the entire synonymy.

As I hope to take up the cataloguing of the Selachii next session, the present list is restricted to the Dipnoi and Ganoidei, including the problematical Acanthodei, which ought perhaps, as in the opinion of many, to be rather considered as Elasmobranchs. The list itself cannot be supposed to be complete, but it will, it is to be hoped, be of a certain utility for a time.

HORIZONS.

The *Upper Old Red Sandstone* (U.O.R.) cannot I think be associated with the Carboniferous rocks on account of its fish remains, which have a greater general resemblance to those of the Lower Old Red of the North of Scotland, in spite of the well-known unconformity which exists between the two sets of strata. In the Carboniferous system two great divisions—Upper and Lower—may be adopted, and these again subdivided according to the plan in use by the Director-general and Officers of the Geological Survey. In this arrangement the Lower Carboniferous falls into two subdivisions, the *Calciferous Sandstone Series* (C.S.) extending up to the first Gilmerton Limestone, and the Carboniferous Limestone Series (C.L.) extending from the last-mentioned horizon to the Millstone Grit, or Rosslyn Sandstone. And in their turn the Upper Carboniferous rocks fall likewise into two subdivisions—the Millstone Grit below, and the Coal Measures (C.M.) above. I have given no contraction for the Millstone Grit, as I have seen no fish remains from this set of rocks in the East of Scotland.

It is certainly worthy of note that in this list not one species is common to the Upper and Lower Divisions of the Carboniferous

formation. So far as fish-life at least is concerned, there certainly occurred an important palæontological break at the base of the Millstone Grit. And my friend Mr Kidston tells me that the same is true with regard to the plants.

LIST.

ORDER DIPNOI.

Family *Ctenodontidæ.*

1. Phaneropleuron Andersoni, *Huxley*.	U.O.R.	Dura Den.
2. Ctenodus interruptus, *Barkas*.	C.S.	Pittenweem, Fife; West Calder.
	C.L.	Gilmerton, Loanhead; Kinghorn.
3. Ctenodus cristatus, *Ag.*	C.M.	Dalkeith.
4. Ctenodus angustulus, *Traq.*	C.L.	Loanhead.
5. Sagenodus quinquecostatus, *Traq.* Syn. *Ctenodus obliquus, var. quinquecostatus,* Traq. *Hemictenodus quinquecostatus,* Traq.	C.L.	Gilmerton, Loanhead.

Family *Uronemidæ.*

6. Uronemus lobatus, *Ag.* Syn. *Phaneropleuron elegans,* Traq.	C.S.	Burdiehouse.
7. Uronemus splendens, *Traq.* Syn. *Ganopristodus splendens,* Traq.	C.L.	Loanhead; Dryden.

ORDER GANOIDEI.

? Suborder ACANTHODEI.

Family *Acanthodidæ.*

8. Acanthodes sulcatus, *Ag.*	C.S.	Wardie; Burdiehouse; Oakbank; W. Calder; Straiton; S. Queensferry; Pitcorthy, Fife; Ardross, &c.

9. Acanthodes Wardi, *Egert.*	C.M.	Edmonstone.
„ sp. indet.	C.L.	Gilmerton, Loanhead.
10. Acanthodopsis Wardi, *Hancock & Atthey.*	C.M.	Smeaton, near Dalkeith.

Suborder PLACODERMI.

Family *Pterichthyidæ.*

11. Bothriolepis hydrophilus (*Ag.*) Syn. *Pterichthys hydrophilus*, Ag.; Egert. *Pamphractus hydrophilus*, Ag. *Pamphractus Andersoni*, Ag. *Homothorax Flemingii*, Ag.	U.O.R.	Dura Den; Drumdryan.

Suborder CROSSOPTERYGII.

Family *Holoptychiidæ.*

12. Holoptychius Flemingii, *Ag.* Syn. *Holoptychius Andersoni*, Ag. *Platygnathus Jamesoni*, Ag.	U.O.R.	Dura Den.
13. Holoptychius nobilissimus, *Ag.* Syn. *Holoptychius Murchisoni*, Ag. *Gyrolepis giganteus*, Ag.	U.O.R.	Dairsie; Drumdryan, Fife.

Family *Rhizodontidæ.*

14. Rhizodus Hibberti (*Ag.*) Syn. *Megalichthys Hibberti*, Ag., pars. *Phyllolepis tenuissimus*, Ag.	C.S.	Wardie; Burdiehouse; S. Queensferry; St Andrews; Straiton; Pitcorthy.

Syn. *Holoptychius Hibberti*, Ag., pars. *Rhizodus gracilis*, M'Coy.	C.L.	Gilmerton; Loanhead; The Moat; Penstone; Bo'ness; Lochgelly; Denhead.
15. Rhizodus ornatus, *Traq.*	C.S.	Burdiehouse; Straiton; Pittenweem; Pitcorthy.
Syn. *Megalichthys Hibberti*, Ag., pars.	C.L.	Gilmerton; Denhead.
Holoptychius Hibberti, Ag., pars.		
Rhizodus Hibberti, Traq., pars.		
16. Archichthys sulcidens, *Hancock and Atthey.*	C.M.	Smeaton.
17. Archichthys Portlocki (*Ag.*)	C.S.	Pittenweem; Abden.
Syn. *Holoptychius Portlocki*, Ag.		
18. Strepsodus sauroides, (*Binney*).	C.M.	Edmonstone; Pirnie Colliery, Leven.
Syn. *Holoptychius sauroides*, Binney.		
19. Strepsodus striatulus, *Traq.*	C.L.	Loanhead; Abden.
20. Strepsodus minor, *Traq.*	C.S.	Pitcorthy.
21. Rhizodopsis sauroides, (*Williamson*).	C.M.	Smeaton; Edmonstone.
Syn. *Holoptychius sauroides*, Williamson.		
Rhizodus granulatus, Salter.		
22. Rhizodopsis, sp. indet.	C.L.	Loanhead.

Family *Saurodipteridæ.*

23. Glyptopomus minor, *Ag.*	U.O.R.	Dura Den.
24. Glyptolæmus Kinnairdi, *Huxley.*	U.O.R.	Dura Den.
Syn. *Diplopterus Dalgleishiensis*, Anderson.		
25. Megalichthys laticeps, *Traq.*	C.S.	Burdiehouse; Burntisland.

26.	Megalichthys lævis, *Traq.*	C.S.	Straiton.
27.	,, Hibberti, *Ag.*	C.M.	Smeaton; Shawfair.
	,, sp. indet.	C.L.	Gilmerton, Denhead.
	Family *Cœlacanthidæ.*		
28.	Cœlacanthus lepturus, *Ag.*	C.M.	Smeaton.
	,, sp.	C.L.	Abden.
	Suborder ACIPENSEROIDEI.		
	Family *Palæoniscidæ.*		
29.	Elonichthys nemopterus, (*Ag.*) Syn. *Amblypterus nemopterus*, Ag. *Amblypterus punctatus*, Ag. pars. *Palæoniscus Robisoni*, Hibbert. *Palæoniscus striolatus*, Ag. *Elonichthys intermedius*, Traq. *Elonichthys ovatus*, Traq. ,, *Dunsii*, Traq. *Elonichthys tenuiserratus*, Traq.	C.S. C.L.	Wardie; Burdiehouse; S. Queensferry; Straiton; Pumpherston; W. Calder; Broxburn; Juniper Green; Burntisland; Pitcorthy. Gilmerton; Loanhead; Wallyford; Denhead.
30.	Bucklandi (*Ag.*) Syn. *Pygopterus Bucklandi*, Ag.	C.S.	Burdiehouse; Wardie; Straiton; S. Queensferry; Burntisland.
		C.L.	Loanhead.
31.	pectinatus, *Traq.*	C.S.	W. Calder; Oakbank; Straiton.
		C.L.	Loanhead; Gilmerton; Kingseat; Abden; Denhead.
32.	multistriatus, *Traq.*	C.L.	Gilmerton; Loanhead.
33.	striatus (*Ag.*)	C.S.	Wardie; Burdiehouse; Juniper Green; Straiton; Pitcorthy.

Syn. *Amblypterus striatus*, Ag. *Cosmoptychius striatus*, Traq.		
34. Rhadinichthys ornatissimus (*Ag.*) Syn. *Palæoniscus ornatissimus*, Ag. *Rhadinichthys lepturus*, Traq.	C.S.	Burdiehouse; Wardie; Straiton; S. Queensferry; Burntisland.
35. Rhadinichthys carinatus (*Ag.*), Syn. *Palæoniscus carinatus*, Ag., *Rhadinichthys Geikiei*, Traq.	C.S.	Wardie, Pumpherston, Pitcorthy, Colinton; Redhall; Corn Ceres, near Kilrenny.
36. Rhadinichthys brevis, *Traq.*	C.S.	Wardie.
37. Rhadinichthys tenuicauda, *Traq.*	C.L.	Wallyford.
38. Rhadinichthys ferox, *Traq.*	C.S.	Wardie.
39. „ macrocephalus, *Traq.*	C.S.	Pumpherston.
40. Nematoptychius Greenockii, *Ag.* Syn. *Pygopterus Greenockii*, Ag.,	C.S.	Wardie; Burdiehouse; Burntisland; Straiton; Oakbank; W. Calder; Juniper Green.
Pygopterus elegans, C. W. Peach. *Nematoptychius gracilis*, Traq.	C.L.	Gilmerton; Loanhead.
41. Acrolepis semigranulosus *Traq.*	C.S.	Straiton.
42. Gonatodus punctatus, *Ag.*, Syn. *Amblypterus punctatus*, Ag. pars. „ *Amblypterus anconoæchmodus* Walker.	C.S.	Wardie; Straiton; Burdiehouse; Pitcorthy.
43. Gonatodus macrolepis, *Traq.*	C.L.	Gilmerton.

44. Gonatodus parvidens, *Traq.*	C.L.	Loanhead; Lochgelly.
45. Drydenius insignis, *Traq.*	C.L.	Loanhead.
46. Cryphiolepis striatus, *Traq.* Syn. *Cœlacanthus striatus*, Traq.	C.L.	Loanhead.
Family *Platysomidæ.*		
47. Eurynotus crenatus, *Ag.* Syn. *Eurynotus fimbriatus*, Ag.	C.S.	Wardie; Burdiehouse; Juniper Green; Craigleith; S. Queensferry; Pumpherston; Burntisland; Pittenweem; Pitcorthy; Kenlymouth; Corn Ceres, near Kilrenny.
	C.L.	Gilmerton; Loanhead; Abden; Denhead.
48. Eurynotus microlepidotus, *Traq.*	C.L.	Loanhead.
49. Platysomus parvulus, *Ag.*	C.M.	Edmonstone; Smeaton.
50. Cheirodus crassus, *Traq.*	C.L.	Abden.

Notes on Some of the Species in the Preceding List.

Acanthodes sulcatus, Ag.—The type specimen from Wardie, in the Oxford Museum, is so poor a fragment that the reference to this species of the now pretty numerous and tolerably perfect examples of *Acanthodes* from the Calciferous Sandstone Series of the district is more a matter of guess-work than of absolute scientific proof, though at the same time there is no real reason for doubt. The fish attains to a very considerable size, and is very closely allied to *A. Wardi*, Eg., of the Coal Measures. The little "sulcus" or groove on the scales, figured by Agassiz, and upon which he founded the specific name, is very inconstant, and can hardly be used as a character; in fact, the only tangible mark which I can find to distinguish *sulcatus* from *Wardi* is a somewhat greater straightness and slenderness of the "styliform" bone.

I am of opinion that the remains of *Acanthodes* which occur in

the Carboniferous Limestone Series are specifically distinct both from *Wardi* and *sulcatus*, but unfortunately they are not sufficiently perfect to warrant the application of a new name.

Sagenodus.—At the suggestion of Mr Smith Woodward, I adopt the term *Sagenodus*, Owen, for Ctenodont fishes of the type of *obliquus*, Atthey, and *quinquecostatus*, Traq., in place of *Hemictenodus*, Jaeckel. *Sagenodus inequalis* seems pretty certainly to have been founded by Sir R. Owen on a microscopic section of a young specimen of *Ctenodus obliquus*, Atthey (*Trans. Odontolo. Soc.*, vol. v., 1867, p. 395, pl. xii.), while on the other hand one cannot feel sure that Jaeckel was correct in referring Atthey's species to his genus *Hemictenodus.*

Phaneropleuron.—I include *Phaneropleuron* with *Ctenodus* and its allies in the family Ctenodontidæ, because the recent observations of Whiteaves, Jaeckel, and myself on specimens of *Ph. curtum*, Whiteaves, from the Upper Devonian of Scaumenac Bay, Canada, clearly show what I had long suspected in the case of *Ph. Andersoni*, Huxley, that the configuration of the palatal bones and palatal dentition was essentially the same as in *Ctenodus* and *Dipterus.* Marginal teeth have, it is true, been also described by Huxley in *Ph. Andersoni* and by Whiteaves in *Ph. curtum*, but I do not consider that this character excludes it from the family. The name Ctenodontidæ is, I think, preferable to Pander's "Ctenododipterinæ," as *Dipterus* is the only member of the family known to us in which the dorsal fin is differentiated into two.

Uronemidæ.—Notwithstanding the great external resemblance of *Uronemus* to *Phaneropleuron*, I am constrained to put the former in a distinct family, owing to the fact that the dentition does not assume the form of "ctenodont" plates, and the shape of the palatal bones is quite different. In *Uronemus* these bones are broad plates, with only a row of teeth along the outer margin, the surface internal to which is simply granulated. Details as to the structure of *Uronemus* will be given in a subsequent memoir.

Strepsodus minor, n. sp.—Scales about $\frac{7}{8}$ inch in length by $\frac{1}{2}$ inch in breadth; usually more or less quadrate or rectangular in aspect, the upper and lower borders being straight and parallel, the posterior border gently convex. Covered surface showing fine concentric striæ, exposed area with radiating raised lines or feeble ridges, indicative

of subjacent vascular channels. Associated with these scales is a small tooth, shaped as in *Strepsodus sauroides*, but with the striation of a much more delicate character (Museum of Science and Art).

Megalichthys lævis, n. sp.—I apply this term to a comparatively small species of *Megalichthys*, whose more or less disjointed remains are not uncommon in the ironstone nodules contained in the "roof" of the Dunnet oil shale worked at Straiton and Pentland. Its distinguishing specific character is the thinness of the scales and the absence of the usual prominent rib or keel on the under surface. In fact, these scales, when seen from below, are almost indistinguishable from those of *Rhizodopsis*. Externally they are brilliantly ganoid on the free portion of the surface, as in other species. Type specimens in the Museum of Science and Art, Edinburgh.

Elonichthys nemopterus, Ag.—After puzzling for years over innumerable specimens of the *Robisoni* type of *Elonichthys*, which is so common in the Lower Carboniferous Rocks of central Scotland, and in vain seeking for definite characters towards dividing them into "good" species, I have been reluctantly compelled to abandon the quest, and to seek for a solution of the question by reuniting them all, with the exception of *E. Bucklandi* (Ag.), which can always be easily recognised by the strongly-marked and deeply-cut ornament of its scales.

It is no doubt easy enough to put together a set of extreme forms, which any one might readily be tempted to adopt as distinct species, and in fact I did so myself at the commencement of my investigations; but the more material I obtained, the more and more unreliable did I find every character turn out upon which I had fixed as diagnostic. For example, the relative fineness or coarseness of the fin-rays, and the relative distance of their transverse articulations, are characters which are quite inconstant. As a rule, the fin-rays are proportionally more slender and more distantly articulated in young specimens, though this condition sometimes persists in adult forms. Also, no reliance can be placed on the relative extent to which the scales are striated or punctate, or upon whether the punctate area is nearly smooth or thickly covered with punctures. Unfortunately the relative size of the scales and the number of rays in each fin are characters which can only be accurately ascertained in exceptionally well-preserved specimens, as a certain amount of

distortion is common to the greater number of those specimens as they occur in the rock.

All the forms which I include under *Elonichthys nemopterus* are fishes of a tolerably deeply fusiform shape, with large fins, the dorsal and anal triangular, and high in front; the fin rays longitudinally striated, save in some cases the proximal parts of those of the lower lobe of the caudal; the principal rays of the pectoral articulated up to their origins. The scales are finely serrated on their posterior margins; their ornament is delicately striato-punctate, as shown in my description and figures of those of the variety *striolatus* (Carb. Ganoids, *Pal. Soc.*, 1877); but, as remarked above, there is an infinite variety in the relative extent of the striation and punctation. As a rule the most anterior scales are entirely striated; those of the middle of the body striated towards the anterior margin and punctate posteriorly; while those towards the tail become nearly smooth. The cranial roof bones are for the most part finely tuberculated; those of the face striated, and very considerable variety occurs here as to the closeness and prominence of the striation.

As well marked *varieties* I may retain the following:—

a. *E. nemopterus*, Ag. type.—Fin-rays slender, rather distantly articulated, striated ornament prevalent on the scales. *C.S.*, Wardie, Pumpherston.

b. Var. *striolatus*, Ag. — Fin-rays relatively coarser, *very closely articulated*, scales delicately striate-punctate. The *Palæoniscus Robisoni* of Hibbert and Agassiz (*Elonichthys Robisoni*, Traq. *olim*) is doubtless the young of this variety. *C.S.*, Burdiehouse, Burntisland, Pitcorthy.

c. Var. *intermedius*, Traq.—Like *striolatus*, but the transverse articulations of the median fin-rays not so close. *C.S.*, Wardie, Burdiehouse, S. Queensferry, Pitcorthy, &c. This includes one of the two types of Agassiz's *Amblypterus punctatus* from Wardie.

d. Var. *Dunsii*, Traq.—Rays of the dorsal fin finely serrated posteriorly. *C.S.*, Broxburn.

e. Var. *tenuiserratus*, Traq.—Scales with the external ornament very delicately marked, nearly smooth, posterior serration very fine. Sculpture of head bones strongly marked. Of all the forms enumerated above, this is the one I abandon

with the most reluctance, but the occurrence of links apparently connecting it with the others, leaves me no alternative. *C.S.*, W. Calder.

f. Var. *affinis*, Traq.—Like *intermedius*, but the fins apparently rather smaller and composed of fewer rays. *C.L.*, Gilmerton, Loanhead, Wallyford, Denhead.

As to *E. ovatus*, Traq., I abandon it altogether, as being pretty certainly a form with the body shortened up by distortion.

Elonichthys multistriatus, n. sp.—The remains of this remarkable and undoubtedly new species which have as yet been found, consist of fragments of fishes more or less distorted, and also disjointed scales and bones. The former set of remains indicate that the body was deeply fusiform, the fins large and composed of numerous closely striated rays, and the head bones resembling in shape those of *E. pectinatus*, Traq. But the scales are altogether peculiar. Those of the flanks are higher than broad, more or less rectangular in form, the covered area narrow, the exposed portion covered with fine sharp raised striæ or ridges, which are sub-parallel, frequently bifurcating and also anastomosing, and cross the surface of the scale obliquely from above downwards and backwards. From the upper margin of the scale, and rather near the anterior superior margin, there projects a strong pointed and grooved articular spine; the posterior margin is entire and without serrations.

When I first saw the scales of this fish, their form and manner of ornament strongly suggested Platysomid affinities, but the maxilla and cast of the mandible lying with them in the same slab clearly showed that we had to deal with a member of the Palæoniscidæ. Other specimens, as aforesaid, indicate that the safest genus to place it in is *Elonichthys*. It must have attained a considerable size.

C.L., Gilmerton and Loanhead. Type specimens in the collection of the author.

Elonichthys striatus, Ag.—I formerly separated this species under the generic term *Cosmoptychius*, owing to the presence of a small plate, which is wedged in anteriorly between the operculum and the quadrate plate beneath it, usually reckoned as suboperculum.

As in the Lower Permian genus *Rhabdolepis* of Troschel, this plate extends back the whole way to the opercular margin, and completely separates the operculum from the quadrate plate beneath. I considered it as the true "suboperculum," and reckoned the quadrate plate as "interoperculum," an opinion which I subsequently recanted in my paper on *Chondrosteus*,* in which I restored the term "suboperculum" to the quadrate plate, and considered the small intermediate one as accessory. I had not, at the time I instituted the genus *Cosmoptychius*, observed this little triangular accessory plate in *Elonichthys*, but since then I have seen it frequently, and in consequence I feel that there is no sufficient ground for separating *striatus* from the other species of the last named genus.

Rhadinichthys ornatissimus, Ag.—I have long had no doubt as to my former *R. lepturus* from Burntisland being only a young specimen of *R. ornatissimus*, and therefore cancel the species. Exceptionally fine specimens of *R. ornatissimus* have recently occurred in the roof of the Dunnet shale at Straiton and Pentland.

Rhadinichthys carinatus, Ag.—I much regret that a similar fate must befall *R. Geikiei* from Redhall; but the accession of a fine series of *carinatus* from Pumpherston has so much added to our knowledge of its characters, that it has become clear that the little specimens from the former locality, which I named in honour of the Director-general of the Geological Survey, must be absorbed in the Agassizian species. In Agassiz's type of this species from Wardie, as well as in other specimens from that locality, the outer surface of the scales is imperfectly shown, hence I formerly described it as entirely smooth or nearly so; but the Pumpherston specimens, which I cannot avoid referring to *carinatus*, from the general appearance and contour of the body and fins, clearly display the same character of scale ornament as I had previously attributed to *Geikiei* (*Proc. Roy. Soc. Edin.*, ix. 1877, p. 439). I also, in my paper on the *Eskdale Ganoids*, doubtfully identified the common *Rhadinichthys* of the Glencartholm beds with the supposed species, *R. Geikiei*, and it has been long evident to me that this doubt was amply justified. Though closely allied, the Glencartholm fish has a considerably coarser serration of the posterior margins of the

* *Geol. Mag.*, June 1887, p. 253.

scales, which is very constant in spite of the numerous other variations exhibited by the large series of specimens which I have examined; and the name *Geikiei* having been originally applied in error to another species, it cannot now be adopted for any member of the genus, so that for the more southern species I must adopt the name *elegantulus*, previously given by me to one of its varieties. *R. delicatulus*, Traq., from the same beds, is also a mere variety of the same species.

Rhadinichthys macrocephalus, n. sp.—Allied to *R. carinatus* in general appearance and scale sculpture, but proportionately shorter, with a smaller number of transverse scale-bands, the flank scales rather high in proportion to their breadth, the head proportionately larger, and the suspensorium less oblique than in the species last referred to. *C.S.*, Pumpherston; the most common species in the "curly shale" worked there and at the adjacent oil work of Holmes. Type specimens in the collection of the author.

Nematoptychius Greenocki, Ag.—*N. gracilis*, Traq., from the Gilmerton Black Band Ironstone, is certainly a young individual of this species.

Acrolepis semigranulosus, n. sp.—From the roof of the Dunnet shale at Straiton, the Edinburgh Museum possesses a slab covered with scales of a large *Acrolepis*, the sculpture of which is different from that of any other member of the genus with which I am acquainted. These scales, which have the size and form of those of *A. Hopkinsi* (M'Coy), are covered on their exposed area with innumerable closely-set fine ridges, often tortuous, and tending constantly to break up into tubercles; their main direction is, however, as usual, obliquely across the scale from the anterior margin.

As my *Elonichthys ortholepis* from Glencartholm turns out to be a young specimen of a large *Acrolepis*, there are now four species of *Acrolepis* known, or at least described, from the Carboniferous rocks of Great Britain, viz.,—*Hopkinsi*, M'Coy; *Wilsoni*, Traq.; *ortholepis*, Traq.; and *semigranulosus*, Traq.

I may here mention that I can see no radical distinction between the scales of the fish from the Carboniferous Limestone series of the West of Scotland, which I described as *Acrolepis Rankinei* (Ag.), and the scales from Derbyshire in the Woodwardian Museum, Cambridge, figured by M'Coy as *Holoptychius Hopkinsi*. Though,

according to the traditions of the Glasgow geologists, this species is Agassiz's *Gyrolepis Rankinei*, the name, having been accompanied by no description in the *Poissons Fossiles*, has no right of priority.

Drydenius insignis, n. gen. and sp.—When my attention was first drawn to the vertebrate fossils of the Borough Lee and Loanhead Blackband Ironstone, I attributed certain small jaw-fragments, showing peculiar bent cylindro-conical and conspicuous teeth, to *Gonatodus macrolepis*, Traq.; but after collecting a large number of those little dentigerous bones, it began to be clear to me that I had, on the other hand, to deal with a new fish, of which more or less entire specimens with the teeth *in situ* began also to turn up.

The most entire example I have seen is 4 inches in length, and, but for the peculiarity of the dentition to be presently described, one would indeed be inclined to refer it to *Gonatodus*. The scales are exactly, as in that genus, nearly smooth, with finely serrated posterior margins; the fin-rays are proportionally rather coarse and also smooth; the cranial roof bones are ornamented with flattened tortuous ridges. It is in the bones of the jaws that the peculiarities reside, which have induced me to elevate the species also to the type of a genus. The hinder part of the maxilla forms a short expanded plate, from the *middle* of the anterior aspect of which the narrow anterior or suborbital process extends, so that the tooth-bearing margin is posteriorly bent suddenly downwards at a considerable angle. This margin is set as in *Gonatodus* with a single row of proportionally stout cylindro-conical pointed teeth. The dentary element of the mandible is rather stout, and shows on its upper margin a row of similar teeth. The splenial element presents a dental armature which I have not seen in any other palæoniscid. The bone is narrow, rounded posteriorly, concave externally, and tapering to a point in front, its upper straight margin being set with a single row of short conical pointed teeth. But the inner, or oral aspect, shows an area about the middle, and occupying more than ⅓ of its length, from which a row of six powerful cylindro-conical teeth arises, behind which are three or four small ones. The large teeth seem disproportionally large for the small size of the fish, and are conspicuous even in the most crushed heads. They are strongly curved with the convexity inwards.

As already mentioned, I have seen no such dentition either in *Gonatodus* or in any other palæoniscid fish, and consequently I have considered it advisable to constitute the new genus, *Drydenius*, for this little fish. The name is taken from the Vale of Dryden, which is in close proximity to the ironstone mines of Borough Lee and Loanhead. Type specimens in the Edinburgh Museum of Science and Art.

Eurynotus crenatus, Agassiz.—The original specimen of *E. fimbriatus*, Ag., from Wardie, in the Oxford Museum, is a very poor fragment, upon which no specific characters distinguishing it from *E. crenatus* can really be founded, especially if one takes into account the enormous number of other specimens of the genus from the Scotch Lower Carboniferous rocks which have been collected since Agassiz wrote the *Poissons Fossiles*. These specimens show very great differences in many respects; but the result of my puzzling over them for years is that, with the exception of a peculiar form from Loanhead, all must be referred to the same species, namely, *E. crenatus*, Ag. In some cases the scales are comparatively smooth, in others more ornamented; in some the crenation of their edges in the dorsal region is extremely coarse, in others this character is not so marked. With regard to the fin-rays, the same remarks apply which I made in the case of *Elonichthys nemopterus*, namely, that in young specimens these are more slender and the transverse joints more distant than in adults, in which also there is no fixed condition as regards this particular. In many instances the rays of the dorsal fin are serrated posteriorly, but I cannot venture to found a species on this.

Eurynotus microlepidotus, n. sp.—Characterised by the small size of its scales and the large dimensions of its fins. *C.L.*, Loanhead. Type specimens in the collection of the author.

Cheirodus crassus, n. sp.—Scales with a relatively coarser ornament than in *Ch. granulosus*, Young; internal rib or "lepidopleuron," not nearly so distinctly marked off; *C.L.*, Abden. The same scales occur at Beith, Ayrshire, associated with dentary plates referable to *Cheirodus*. Type specimens from Abden in the Museum of Science and Art. From Beith, in the collection of Robert Craig, Esq., Langside, Beith.

The Interactions of Circular and Longitudinal Magnetisations. By Prof. C. G. Knott, D.Sc.

(Read July 21, 1890.)

Preliminary Note.

In the course of an extended series of investigations into the relations of magnetism and stress, part of which is already published in the *Transactions* (vol. xxxv.), I was led to consider the effect of a current passing along a wire upon its longitudinal magnetic intensity. Wiedemann, Buff, and Villari * have discussed this problem in some of its aspects. It does not seem possible, however, to deduce from their results completely satisfactory conclusions as to the effect of the current upon the apparent longitudinal permeability. The question, as it presented itself to my mind, was not so much as to the effect of a varying current along the wire upon the apparent longitudinal moment, but rather as to the behaviour of the wire in various longitudinal fields according as it was carrying a current or not. It was to be expected, in accordance with the results of previous investigators, that a diminution of longitudinal intensity would be an evident accompaniment of a steadily sustained circular magnetisation. In other words, the susceptibility to a longitudinal magnetising force would, in all probability, be smaller when a current was passing along the wire. This has been fully established in the experiments, of which this forms a short preliminary note. Other effects, however, have been observed, which are (so far as I know) novel, and which may lead to clearer views regarding the internal structure of magnets and magnetised matter.

In the experimental work I have been assisted by Mr Tsuruta, a graduating student of physics in the Imperial University.

The wire to be experimented with was carefully annealed, and then inserted as the core of a solenoid of copper wire of 138 coils per centimetre length. The wire and solenoid lay magnetically east and west, and at a suitable distance from the one end a delicately suspended mirror with small magnets attached to its back served in the usual way as the magnetometer. The deflections were measured by the familiar reflected beam method, the spot of light moving

* For references, see Wiedemann's *Die Lehre von der Electricität*, vol. iii. pp. 456–462; for theoretical discussion, see p. 476.

over a scale at a distance of 1·7 metres from the magnetometer mirror. Before the wire was inserted, the electromagnetic action of the coil on the magnetometer was corrected by a small adjustable coil in circuit with the solenoid and set close to the magnetometer. The wire was then inserted and subjected to cyclic variations of magnetic force. The magnetising current was changed continuously from a given positive value to an equal negative value, and back again to the original positive value. The variation was effected in a gradual manner by means of a liquid rheostat, consisting of a column of dilute sulphate of zinc with zinc electrodes, through which a steady current from a battery of Daniell cells was constantly flowing. By means of a sliding zinc electrode, the necessary current was shunted through the solenoid circuit. The tangent galvanometer included in this circuit was, of course, far removed from the solenoid, and was so placed that the operator in adjusting the rheostat could easily read the galvanometer deflection.

In the earlier experiments careful attention was paid to the first effects, as well as to the permanent cyclic condition, which soon becomes established after a few cyclic changes of the magnetising current have been gone through. At suitable stages in the variation of the current, the current was kept steady until the corresponding deflection of the magnetometer needle was observed and noted. These remarks will suffice at present to indicate the general method adopted in studying the cyclic changes which have been so fully investigated by Warburg and Ewing.

One of my objects was to study the modification produced on this cycle, and on whatever else may be associated with it, when a current is passed along the magnetised wire. This linear current (as we shall call it) was derived from one or more Bunsen cells. It entered the iron wire at the end furthest removed from the magnetometer, and returned along two copper wires stretched parallel to the iron wire, and very close to it.

A complete set of experiments consisted in—

(1) Taking a permanent magnetic cycle when no linear current was flowing along the wire;

(2) Observing the initial effect when the linear current was made to flow in one direction along the wire, and taking the permanent magnetic cycle, with this current kept steady;

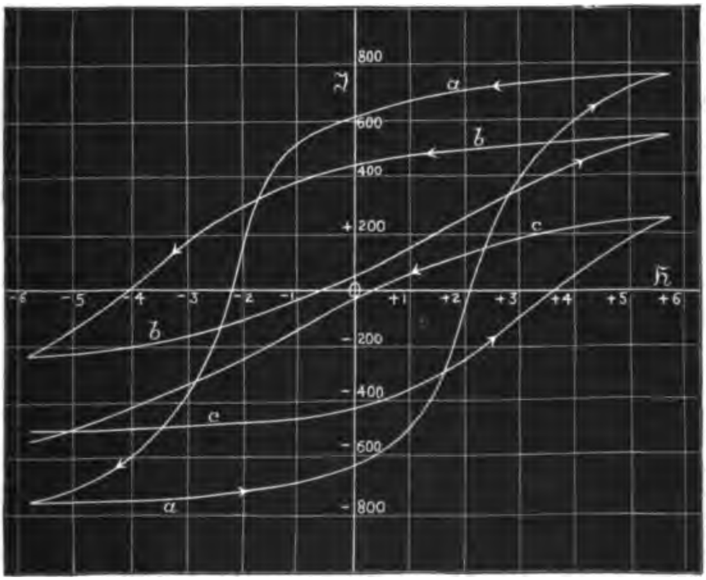
(3) Observing the initial effect when the linear current was broken, and taking again the permanent magnetic cycle, as in (1);

(4) Observing the initial effect of the linear current put on in the other direction, and taking the permanent magnetic cycle with this current steady;

(5) Operating as in (3).

It was soon found that in good experiments the permanent magnetic cycles in (1), (3), and (5) were practically identical; so that in later experiments (3) was omitted. It was also established by direct experiment that the permanent magnetic cycle in (2) was quite independent of the magnetic condition of the wire when the linear current was put on.

Some of the features of the case are shown in the curves given. This is chosen as an average type, the departures from which, when the relative magnitudes of linear current and field are altered, will



be briefly indicated below. In these curves magnetic force ($\mathfrak{H}$) is measured horizontally; magnetic intensity ($\mathfrak{I}$) measured vertically. The magnetic force is given in electromagnetic *c.g.s.* units, and the magnetic intensity (very roughly) in the same.

Curve (*a*) corresponds to the permanent magnetic cycles (1), (3), (5); curves (*b*) and (*c*) give the cycles when the linear current is flowing first in the one and then in the other direction. The current used in this experiment was about 2 ampères.

A glance at these curves is sufficient to teach us that the linear current modifies the properties of iron in relation to the magnetic after-effect in three well-marked ways.

First, the total range of magnetic intensity produced by a given cyclic variation of magnetic force is markedly diminished when the current is flowing along the wire. This means a diminution in susceptibility. Here also may be mentioned the fact that the first effect of putting on the current when the wire is strongly magnetised is a diminution of longitudinal magnetic intensity.

Secondly, when the linear current is flowing, the average magnetic intensity over a whole cycle no longer corresponds to the condition of zero polarity, as in the normal case, when no linear current is flowing. For the one current the magnetic intensity oscillates, so to speak, about a large positive polarity; and, for the current in the reverse direction, it oscillates about a nearly equal negative polarity. If we reckon polarity in the usual way, as a directed quantity measured from the south pole to the north, then the direction of the linear current is in the same direction as the average polarity which it sustains, when the wire is subjected to an absolutely symmetrical cycle of magnetising force on each side of zero.

Thirdly, the curves (*b*) and (*c*) are no longer symmetrical on the side of the zero line of magnetic force. The closed curve (*b*) is more pointed at its positive end than at its negative end; the curve (*c*), on the other hand, is more pointed at its negative end than at its positive. If we turn the figure upside down, the general appearance remains the same as before. Thus, the effect of the linear current is such as very distinctly to modify the form of the ascending and descending branches. The descending branch of (*b*) is very similar to the ascending branch of (*c*); and the ascending branch of (*b*) is very similar to the descending branch of (*c*). We may connect this peculiarity with the peculiarities already described in these words:—

A current passing along an iron wire, which is being magnetised, diminishes the apparent susceptibility of the wire; but this effect

is more pronounced when the wire is acquiring a longitudinal polarity in the opposite direction to that in which the linear current is flowing. Hence, during any cyclic operation, the wire tends to acquire an average polarity in the same direction as that in which the current flows.

These effects are more pronounced the stronger the linear current is as compared with the magnetising field. In a moderate cyclic field, the effect of the linear current may be so strong as to prevent the wire ever acquiring other than one kind of polarity. For example, this effect was produced in a field $\pm$ 4, with a current of nearly 2 ampères. For the linear current in one direction the (*b*) curve never dipped below the zero intensity line; and for the current in the other direction the curve (*c*) never rose above it.

On the other hand, in stronger magnetising fields, from 10 and upwards, the (*b*) and (*c*) curves for the same linear current of 2 ampères tend to *terminal* coincidence, but diverge at the intermediate stages. It is proposed to study more fully these relations, and to extend the investigations to nickel and cobalt.

In the course of these experiments, an effect was noticed, which demonstrates the extraordinary complexity of magnetic distribution in a magnetised wire. If, before the wire has been magnetised at all, a current is passed along it, no appreciable longitudinal polarity is produced, as measured by a magnetometer needle placed as in the above experiments. Suppose, however, that the wire has been pretty strongly magnetised; and that, in the manner discussed by Auerbach, and used almost universally now, the wire is demagnetised by reversals of gradually diminishing magnetising currents until the magnetometer needle stands almost exactly at zero. It is, of course, generally recognised that the wire so demagnetised cannot be regarded as being in anything like the same condition as it was in its originally unmagnetised state, although it appears to have lost polarity. That this view is correct may be at once demonstrated by simply passing a pretty strong current along the wire, when a very pronounced polarity will be evidenced by a comparatively large deflection of the magnetometer needle. Reversing the current will reverse this polarity. Heating to a red heat can alone truly demagnetise an iron wire.

Closing Address. By The Hon. Lord M'Laren.

(Read July 21, 1890.)

As the Chairman for this evening, I have been asked by the Council to lay before you a brief summary of the events of the session, which will conclude the public business of the session.

In this retrospective view the most interesting point is the number and variety of the papers which have been read. Of these there were in all 74 papers, of which 19 were in the department of Natural Philosophy, 16 in Mathematics, 11 in Chemistry, 1 in Geology, 9 in Zoology, 1 in Botany, 6 in Physiology, 2 in Anatomy, 5 in Meteorology, 2 in Physical Geography, and 2 in Philology. These contributions, I may say, in the opinion of competent judges, are calculated to maintain the reputation of our publications for originality and accuracy. In the present session, moreover, we have had two important contributions in the department of philology and literature, for which we are indebted to Professor Blackie. In these papers the author emphasised the remarkable fact that the Greek language, as now spoken and written, is substantially the same language as the Greek of Lucian and Plutarch, having undergone no material change during the elapse of 2000 years. A late learned and enthusiastic French Hellenist, Monsieur d'Eichthal, held the opinion that Greek (I will not call it Modern Greek, which I take to be a misnomer) will one day take the place of Latin as the medium of correspondence between cultivated men of all countries. Without going quite so far, we may at least point to the vitality of the ancient Greek tongue and its power of adaptation to modern requirements as an answer to the arguments of those who seek to discourage the study of classical literature at our universities, on the ground of the supposed discordance between the ancient and the modern modes of thought and expression.

A more true explanation of the comparative decline of pure scholarship and the preference accorded to the more recondite and perhaps more laborious researches which are necessary for the development of scientific truth, may be found in this observation of M. Renan :—"The intense satisfaction attending scientific work arises from the assurance which the scientist feels that he labours

at work which reposes on an eternal basis of fact, and of which the object at least is eternal,—a work which all civilised nations feel bound to pursue."

There were 12 admissions of new Fellows this session. Last session the admissions amounted to 18; whilst during the three preceding sessions the admissions attained the average number of 36. The high rate, therefore, which characterised those three sessions has not been maintained either in this session or the immediately preceding one. But the admissions have been more than sufficient to maintain the numerical efficiency of the Society, notwithstanding the inevitable diminution of its numbers through death. It is with regret that I have to record the fact that during the past session the Society has lost ten Ordinary Fellows and one Honorary Fellow by death. Of these obituary notices will be forthcoming at the proper time and place, but I may be permitted to say a few words regarding some of the deceased Fellows who are personally, as well as from their public position, known to most of us who are here engaged in carrying on the work of the Society.

Sir Henry Yule, an Honorary Fellow of this Society, was born at Inveresk in May 1820, He was destined for an Indian military career, and in December 1838 he joined the Bengal Engineers. He served in the Sutlej and Punjab campaigns, and during the Mutiny, and was afterwards successively Under-Secretary and Secretary of the Public Works Department in India. In 1875 he was appointed a Member of the India Council. He was editor of the *Travels of Marco Polo* and of many other books of mediæval adventure in Asia. His Glossary of Anglo-Indian Terms is considered a very valuable work.

Andrew Young, a man of true literary and poetic feeling, died on Saturday, the 30th November 1889. He had attended a meeting of this Society on the Monday preceding. After a brilliant career in arts and theology at the university of this city, where he gained in Professor Wilson's class the prize for the best poem on "The Highlands," he was appointed to the Head-Mastership of Niddrie School, and afterwards became English Master in Madras College.

He regularly attended our meetings, and was much esteemed by his friends and fellow-citizens.

Many amongst us will remember the portly presence and genial manner of Patrick Don Swan, who was provost of Kirkcaldy continuously from 1842 to 1886. The celebrated Thomas Carlyle assisted him as tutor in his early life: the friendship of tutor and scholar was unbroken to the last. During the provostship of Mr Swan, Kirkcaldy became prosperous largely through his efforts, and he will long be remembered there for the civic improvements he did so much to effect, and for his munificence to the poor. He died on the 17th December 1889.

It will not be necessary for me to say anything further regarding our much esteemed associate Dr Andrew Graham, as an obituary notice of him has already been communicated to the Society.

James Leslie was educated at the University of Edinburgh, his uncle Sir John Leslie being then professor of mathematics there. It would occupy too much time even to enumerate the various engineering works with which he was connected. These consisted chiefly in the construction of docks, in increasing the water supply of cities, and in fixing the limits of estuaries. Among his other undertakings, he designed a plan for the Monkland Canal, by which empty boats could be conveyed by an inclined plane instead of through locks. This ingenious expedient, which foreshadowed the ship railway, has been much admired by engineers. He was a liberal subscriber to every public purpose. He died on 29th December 1889.

The career of Dr James Lorimer is too well known to render it necessary for me to do more than briefly advert to it here. After studying at the universities of Edinburgh, Geneva, Berlin, and Bonn, he was called to the bar of Scotland in 1845, and was appointed successively to the Principal Lyon Clerkship and to the Chair of Public Law in the University of Edinburgh. His principal works are the *Institutes of Law*, which has been translated into French and Spanish; and his *Institutes of the Law of Nations*. He was the only Scottish Professor to whom the high distinction of an

honorary degree was accorded by the University of Bologna at its octo-centenary in 1888. He died on February 13, 1890.

Sir PETER COATS was Paisley's foremost citizen, esteemed alike for his charitable benefactions to the poor of the town, as well as for his princely gifts to the community. The manufacturing firm of which Sir Peter was at the head employs about 6000 persons. It was to his liberality that Paisley is indebted for the buildings of its Public Library and Museum, costing with the site about £18,000 In recognition of this gift he received the honour of knighthood. He died on 9th March 1890, at Algiers, in the 82nd year of his age.

Dr LEONHARD SCHMITZ was born at Eupen, near Aix-la-Chapelle, in 1807, and settled in England in 1837. In 1844, he published from the notes which he had taken in the class-room at Bonn two volumes of Niebuhr's Lectures on Roman History, for which the King of Prussia awarded to him the great gold medal for literature. In 1846 he was appointed rector of the Royal High School at Edinburgh. He was selected to give lectures in history to the Prince of Wales and to the Duke of Edinburgh, while the Duc d'Aumale, the Prince de Joinville, and the Duc de Nemours placed their sons under his tuition at the High School. After leaving Edinburgh he held various appointments connected with the higher education of the country. He was projector and editor of *The Classical Museum*, wrote several historical works, and contributed largely to the great classical dictionaries, to the *Encyclopædia Britannica* and other publications. He died at the age of 83.

On looking over the work done in the last session, and the papers in our *Transactions* recently published, we cannot fail to remark that some of the most important communications come from the laboratories of the Universities of Edinburgh and Glasgow, and from the laboratory recently founded in this city by the Royal College of Physicians. The Firth of Forth also, which Professor Ray Lankester has called "the classic sea of naturalists," has furnished materials in its fauna for important biological work.

It will be noticed that the elaborate investigations of our Secretary,

Professor Tait, on the Theory of Knot Forms, have induced other scientists in England, in the United States of America, and on the Continent, to follow him in the same department of research, and to give to our Society valuable communications on these subjects. I think we may also attribute to the same source the revival of interest in the abstruse subject of the Quaternion Calculus with which the name of my distinguished friend is largely identified.

It is also pleasing to observe that many papers have been received from comparatively young men, who, after distinguishing themselves at the university, now hold important appointments in distant colonies and dependencies of the Empire and among the old and rapidly advancing nations of the extreme East.

I have now only to announce the conclusion of the public business of the session of 1890.

Meetings of the Royal Society—Session 1889-90.

Monday, 25th November 1889.

General Statutory Meeting. Election of Office-Bearers. *P.* xvii. 1.

Monday, 2nd December 1889.

Prof. Sir Douglas Maclagan, Vice-President, in the Chair.

The following Communications were read:—

1. On the Transformation of Laplace's Coefficients. By Dr G. PLARR. Communicated by Professor TAIT. *T.* xxxvi. 19.
2. Preliminary Note on the Thermal Conductivity of Aluminium. By A. CRICHTON MITCHELL, Esq., B.Sc.
3. On Coral Reefs and other Carbonate of Lime Formations in Modern Seas. By JOHN MURRAY, LL.D., and ROBERT IRVINE, F.C.S. (*With Lantern Illustrations.*) *P.* xvii. 79.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society:—

Prof. RALPH COPELAND, Astronomer-Royal for Scotland.
G. A. Gibson, M.A.

Monday, 16th December 1889.

Sir Arthur Mitchell, K.C.B., LL.D., Vice-President, in the Chair.

A series of Photographs of Stars and Nebulæ, presented by ISAAC ROBERTS, Esq.; and Photographs of Lightning taken in Natal, presented by C. W. METHVEN, Esq., were exhibited.

The following Communications were read:—

1. Note on Cayley's Demonstration of Pascal's Theorem. By THOMAS MUIR, M.A., LL.D. *P.* xvii. 18.
2. On Self-Conjugate Permutations. By THOMAS MUIR, M.A., LL.D. *P.* xvii. 7.
3. On a Rapidly Converging Series for the Extraction of the Square Root. By THOMAS MUIR, M.A., LL.D. *P.* xvii. 14.
4. On some Quaternion Integrals. By Professor TAIT.
5. On the Glissette of a Hyperbola. By the Same. *P.* xvii. 2.

6. On certain Substances, found in the Urine, which reduce the Oxide of Copper upon Boiling in the Presence of an Alkali. By HERBERT H. ASHDOWN, M.D. *P.* xvii. 58.

7. Enzyme Action in the Lower Organisms. By G. E. CARTWRIGHT WOOD, M.D., B.Sc. Communicated by Dr WOODHEAD. *P.* xvii. 27.

8. Observations upon the Structure of a Genus of Oligochæta belonging to the Limicoline Section. By FRANK E. BEDDARD, Esq., M.A. *P.* xvii. 5 (*Abstract*); *T.* xxxvi. 1.

Monday, 6th January 1890.

The Hon. Lord M'Laren, LL.D., Vice-President, in the Chair.

A Photograph of a Group of Sun Spots and of the Sun's Surface, presented by JAMES NASMYTH, Esq., was exhibited.

The following Communications were read:—

1. Obituary Notice of Sir James Falshaw, Bart. By Bailie RUSSELL, B.Sc., M.B. *P.*

2. The Effect of Friction on Vortex Motion. By Professor TAIT.

3. On the Connections of the Inferior Olivary Body. By ALEXANDER BRUCE, M.A., M.D. *P.* xvii. 23.

4. The Internal Condensation of some Diketones. By W. H. PERKIN, D.Sc., Ph.D.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society:—

DAVID HEPBURN, M.B.
WILLIAM HENRY WHITE, F.R.S.

Monday, 20th January 1890.

Sir William Thomson, F.R.S., President, in the Chair.

An Obituary Notice of Dr Andrew Graham, R.N., by JOHN ROMANES, Esq., W.S., was read by the General Secretary.

The following Communications were read:—

1. On Electrostatic Stress. By the PRESIDENT.

2. The Volcanic Eruption at Bandaisan in Japan. By Professor C. MICHIE SMITH. *P.* xvii. 65.

3. On a Curious Set of Fog-Bows. By Professor HEDDLE.

4. On the Nature of a Voluntary Muscular Movement. By J. BERRY HAYCRAFT, M.D., D.Sc. *P.* xvii. 176 (*Abstract*).

Monday, 27th January 1890.

The Rev. Professor Flint, D.D., Vice-President, in the Chair.

The following Communication was read:—

On Evolution and Man's Place in Nature. By Professor CALDERWOOD, LL.D. *P.* xvii. 71.

Monday, 3rd February 1890.

Sir William Thomson, F.R.S., President, in the Chair.

The following Communications were read:—

1. New Estimates of Molecular Distance. By W. PEDDIE, D.Sc.
2. On the Gravimetric Composition of Water. By Prof. DITTMAR.
3. On the Number of Dust Particles in the Atmosphere of certain Places in Great Britain and on the Continent, with remarks on the Relation between the Amount of Dust and Meteorological Phenomena. By JOHN AITKEN, Esq., F.R.S. *P.* xvii. 193.
4. Note on some Barometric Curves in Relation to the Strength of Wind at Ben Nevis Observatory, by Dr ALEX. BUCHAN, was postponed.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society:—

AIKITU TANAKADATE.
The Rev. GEORGE MATHESON, M.A., B.D., D.D.

Monday, 17th February 1890.

Sir William Thomson, F.R.S., President, in the Chair.

The following Communications were read:—

1. On Descartes' View of Space; and Sir William Thomson's Theory of Extended Matter. By S. TOLVER PRESTON, Esq.
2. A New Synthesis of Dibasic Carbon Acids. By Prof. CRUM BROWN, *P.* xvii. 53 (*Abstract*). *T.* xxxvi. 211.
3. On the Electrolysis of Potassium-Ethyl Malonate and of Potassium-Ethyl Succinate. By Prof. CRUM BROWN and JAMES WALKER, D.Sc. *P.* xvii. 54 (*Abstract*).
4. The Action of Sodium Carbonate and Bromine on Solutions of Cobalt and Nickel Salts. By Dr GIBSON. *P.* xvii. 56 (*Abstract*).
5. On the Swimming Bladder and Flying Powers of *Dactylopterus volitans*. By W. CALDERWOOD, Esq. Communicated by Prof. EWART. *P.* xvii. 132.

Friday, 28th February 1890.

Sir Douglas Maclagan, M.D., Vice-President, in the Chair.

The following Communications were read:—

1. Structure and Contraction of Striped Muscular Fibre of Crab and Lobster. By Prof. RUTHERFORD. *P.* xvii. 146 (*Abstract*).
2. The Histology, Functions, and Development of the Carapace of the Chelonia. By JOHN BERRY HAYCRAFT, M.D., D.Sc.
3. Muscular Contraction following rapid Electrical Stimulation of Central Nervous System. By JOHN BERRY HAYCRAFT, M.D., D.Sc. *P.* xvii. 178 (*Abstract*).

Monday, 3rd March 1890.

Sir William Thomson, F.R.S., President, in the Chair.

The following Communications were read:—

1. Note on Ripples in a Viscous Liquid. By Prof. TAIT. *P.* xvii. 110.
2. A Geometrical Method, based on the Principle of Translation. By D. MAVER, Esq. Communicated by Dr. T. MUIR. *P.* xvii. 188.
3. Phases of the Living Greek Language. By Emeritus Prof. BLACKIE. *T.* xxxvi. 45.

Dr JOHN C. M'VAIL was balloted for, and declared to be duly elected a Fellow of the Society.

Monday, 17th March 1890.

Sir William Thomson, F.R.S., President, in the Chair.

The following Communications were read:—

1. An accidental Illustration of the Effective Ohmic Resistance to a Transient Electric Current through an Iron Bar. By Sir WM. THOMSON. *P.* xvii. 157.
2. The Absorption Spectra of Certain Vegetable Colouring Matters. By Prof. C. MICHIE SMITH. *P.* xvii. 121.
3. On the Solution of the Three-Term Numerical Equationof the *n*th Degree. By the Hon. Lord M'LAREN. *P.* xvii. 270.
4. The determination of Surface-Tension by the Measurement of Ripples. By Prof. C. MICHIE SMITH. *P.* xvii. 115.
5. On a Mechanism for the Constitution of Ether: illustrated by a model. By the PRESIDENT. *P.* xvii. 127.

Monday, 7th April 1890.

Professor Ralph Copeland, Ph.D., in the Chair.

The following Communications were read :—

1. Notes on the Solution of certain Equations. By R. E. Allardice, Esq., M.A. *P.* xvii. 139.
2. Some Multinomial Theorems in Quaternions. By the Rev. M. M. U. Wilkinson. Communicated by Professor Tait. *P.* xvii. 149.
3. On the Reflexion-Caustics of Symmetrical Curves. By the Hon. Lord M'Laren. *P.* xvii. 281.
4. An Inquiry concerning Spontaneous Generation. By J. Oliver, M.D.
5. Notes on the Zodiacal Light. By Professor C. Michie Smith. *P.* xvii. 142.
6. Special Integrals of some Linear Partial Differential Equations, with constant Coefficients. By Professor Tait.

Monday, 5th May 1890.

Sir Arthur Mitchell, Vice-President, in the Chair.

The following Communications were read :—

1. Adamantios Koraes, and his Reformation of the Greek Language. By Emeritus Professor Blackie. *T.* xxxvi. 57.
2. The Anatomy of the Antipatharia—Part 2. Schizopathinæ. By George Brook, Esq.

Monday, 19th May 1890.

Professor Chrystal, LL.D., Vice-President, in the Chair.

The following Communications were read :—

1. Obituary Notices : — of
   Dr Edmond Ronalds. By J. Y. Buchanan, Esq. *P.*
   J. J. Coleman, Esq.; Professor Donders. } By Professor M'Kendrick. *P.*
2. On the Synthesis of Sebacic Acid. By Professor Crum Brown and Dr Walker. *P.* xvii. 180 (*Abstract*). *T.* xxxvi. 211.
3. Note on some remarkable Quarternion Formulæ. By Professor Tait.
4. On the Roots of the Auditory Nerve, and their connections. By Dr Alexander Bruce, F.R.C.P.E.

Monday, 2nd June 1890.

Prof. Sir Douglas Maclagan, M.D., Vice-President, in the Chair.

The following Communications were read:—

1. On the Relation of Optical Activity to the Character of the Radicals united to Asymmetric Carbon Atom. By Professor CRUM BROWN, F.R.S. *P.* xvii. 181.

2. On the Mean Level of the Surface of the Solid Earth. By HUGH ROBERT MILL, D.Sc. *P.* xviii. 185.

3. Weierstrass' Contributions to the Calculus of Variations. By J. CROCKETT, Esq. Communicated by Professor TAIT.

4. Barometric Record in the Vicinity of a Tornado. By JOHN ANDERSON, Esq. Communicated by Dr BUCHAN.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society:—

Dr J. J. CHARLES, M.A.
R. L. MOND, B.A. Cantab.

Monday, 16th June 1890.

The Hon. Lord M'Laren, Vice-President, in the Chair.

The following Communications were read:—

1. List of West Australian Birds; showing their Geographical Distribution throughout Australia, including Tasmania. By A. J. CAMPBELL, Esq., Melbourne. Communicated by the Rev. Dr MACGREGOR. *P.* xvii. 304.

2. On a Difference between the Diurnal Barometric Curves at Greenwich and at Kew. By ALEXANDER BUCHAN, Esq., LL.D.

3. On the General Formulæ for the Passage of Light through a spherically-arranged Atmosphere. By E. SANG, Esq., LL.D.

4. On a remarkable Barometric Reading at the Ben Nevis Observatory on April 8, 1890. By ALEX. BUCHAN, LL.D.

5. Synthesis by means of Electrolysis—Part III. Synthesis of *n*-Dicarbodecahexanic Acid. By Professor CRUM BROWN and Dr JAMES WALKER. *P.* xvii. 293.

Monday, 7th July 1890.

Sir William Thomson, President, in the Chair.

PRIZES.

The Gunning Victoria Jubilee Prize for 1887–90 was presented to Professor Tait, for his work in connection with the "Challenger" Expedition, and his other Researches in Physical Science.

The President, on presenting the Prize, said :—

This work was primarily undertaken by Professor Tait, for the purpose of correcting thermometric observations affected in a complex manner by the severe and varying pressures to which the instruments were subjected, and deducing the true temperatures of the water at the great depths explored in the "Challenger" expedition. Even had the investigation been confined to this object alone, it must have included highly interesting researches regarding the thermoelastic qualities of water, mercury, and glass. But a mere knowledge of the temperature at all depths in the seas and oceans has comparatively little interest or importance in science without knowledge of physical properties of sea water, such as compressibility and thermal expansivity at pressures corresponding to all the depths. Particularly the temperature of maximum density for any given pressure, which is a property calculable from the other two, is of supreme importance in explaining the observed results of thermometric determinations in water over submarine slopes and under the levels of the lips of submarine basins, and using the results for the theory of the great oceanic circulations. Accordingly, Tait has included all these subjects in his investigation, which, carried on with exemplarily determined perseverance, and with peculiar skill and inventiveness, through six years, has added to science a valuable body of results regarding the compressibility and expansivity of fresh water and sea water at different temperatures and pressures, the compressibilities of water with different proportions of common salt in solution, and the compressibilities of glass.

Many of us, who have seen something of the work from time to time, will remember with vivid interest the great Woolwich and the small Edinburgh guns, and the wonderful appliances for producing and measuring the enormous pressures up to 457 atmospheres (3 tons weight per square inch) which were used, and their effects on the apparent bulk of mercury in glass. But I am afraid I have misused the word apparent: we saw neither mercury nor glass, but

were told they were there, screened from our eyes by 43 millimetres of steel. Electric contacts made by mercury on fine platinum wires as it rose in the glass tube, forming a long neck to an inverted bottle of water, showed, by electric signals outside the gun, the volumes of the compressed mercury as measured by the less compressed glass measure, with the different pressures shown by a proper pressure measurer at the times of the electric signals. Tait communicated this novel and ingenious method of measuring an unseen liquid by an unseen measuring vessel in a space subjected to a pressure of hundreds of atmospheres to Amagat, at a critical time in the course of his great work when it was much needed, and proved most valuable. In return, Amagat gave Tait the pressure-measuring instrument, which was as great an acquisition to him as Tait's invention was to Amagat.

The Royal Society awards to Professor Tait for this work the Gunning Victoria Jubilee Prize. It will, I am sure, be gratifying to Dr Gunning, the founder of this prize, that so valuable a contribution to science has earned it for the period 1887–90.

The Keith Prize for 1887–89 was presented to Professor Letts, for his Researches into the Organic Compounds of Phosphorus.

Professor Crum Brown, on presenting the Prize, said:—

The Council has awarded the Keith Prize, for the biennial period 1887–1889, to Professor Letts, for his papers on the Organic Compounds of Phosphorus. The work was difficult, and has been very thoroughly done, and the results are of great interest. The special difficulty arose from the nature of the substances to be dealt with, and also from the want of a trustworthy mode of determining the quantity of phosphorus contained in them. The latter difficulty Professor Letts has completely got over, and has devised a method by means of which the phosphorus in these compounds can be accurately determined. The special interest of these investigations depends on the remarkable similarities and equally remarkable dissimilarities shown by the corresponding compounds of phosphorus, nitrogen, and sulphur. It was indeed the desire to trace out these analogies and differences through a large series of compounds that led Professor Letts to undertake what has turned out to be a very laborious piece of work. This steady purpose has given unity to a considerable series of papers, in some of which Professor Letts had the collaboration of his assistant. Messrs Collie, Wheeler, and Blake have thus successively shared in this important investigation.

The Neill Prize for 1886–89 was presented to Mr Robert Kidston, for his Researches in Fossil Botany.

Professor James Geikie, on presenting the Prize, said:—

In Scotland, geological science has been for many years prosecuted chiefly in its physical departments. Nor is this to be wondered at when one considers the generally unfossiliferous character of Scottish rocks, and, on the other hand, the many interesting problems which the intricate structure of our country presents. Dr Hutton, the father of physical geology, is the characteristic product of such a country as this, just as William Smith, the father of historical geology, is the typical product of England, with its long and orderly succession of fossiliferous systems. The richly fossiliferous Mesozoic and Tertiary strata, which occupy so large a part of the sister country, are very sparingly developed here. The palæontologist with us has to content himself with much older strata, which, after all, are only locally fossiliferous, and with a few meagre representatives of younger systems. Limited as are the opportunities for the prosecution of palæontological research in Scotland, we have yet never wanted able investigators in this branch of science. Few of these, however, have worked much in the department of fossil botany. Doubtless this is largely due to the difficulties that beset the study itself. The evidence with which the palæophytologist has to deal is often very hard to understand, and its interpretation demands a wider and more profound knowledge of the structure of living and extinct forms than is generally required in the study of invertebrate palæozoology. When one glances over the contents of a palæontological museum, one can readily understand why the study of extinct animal life has come to be more assiduously cultivated than fossil botany. The contrast between the more or less perfect state of preservation of many of the corals, shells, and other invertebrates, and the fragmentary character of the plant-remains—the flotsam and jetsam of vanished rivers, lakes, and seas—is quite sufficient to account for the preference that is given to palæozoology. When a competent investigator, therefore, devotes himself to the difficult study of fossil botany he is deserving of every encouragement; and when his researches have resulted in eminent success, they must claim the ungrudging appreciation of all who wish well to geological science. The researches in which Mr Robert Kidston has been engaged have made his reputation amongst geologists everywhere. Trained in botanical science by the late Professor Dickson, under whom he studied for some years, Mr Kidston took up the somewhat ungrateful

study of palæophytology, devoting himself more particularly, as might have been expected, to palæozoic botany. In this department of science he has contributed upwards of thirty papers—all of which are characterised by accurate observation and clear definition and description.

One of the principal objects of his studies has been to determine the affinities of palæozoic genera and species with those of existing forms. With this view he has described the fructification of a number of carboniferous ferns and lycopods, and has also given special attention to the arborescent lycopods of the same great system. Another important object he has constantly kept before him has been the working out of the horizontal and vertical distribution of the carboniferous plants of Britain. He has consequently been led to compare the plant-remains of the several British coalfields with each other and with those of the coalfields of other countries, and has thus thrown much wished-for light on the relative age of widely separated areas of carboniferous strata.

Mr Kidston's more important papers have generally appeared in the *Transactions* of this Society. The first of the series, dealing with the fossil flora of definite areas, was his "Report on the Fossil Plants collected by the Geological Survey in Eskdale and Liddesdale" (*Trans. Roy. Soc. Edin.*, vol. xxx.), which was followed at intervals by papers on the "Fossil Flora of the Radstock Series of the Somerset and Bristol Coalfield" (*Ibid.*, vol. xxxiii.), "On some Fossil Plants from Teilia Quarry" (*Ibid.*, vol. xxxv.), and "On the Fossil Flora of the Staffordshire Coalfields" (*Ibid.*, vol. xxxv.). Several papers treating of the fructification of ferns have likewise appeared from time to time in the publications of this Society and the Royal Physical Society, as also in the *Annals and Magazine of Natural History;* while the same subject has been dealt with by Mr Kidston in his papers descriptive of the flora of various coalfields. His wide and accurate knowledge of his subject was recognised by the authorities of the British Museum, who requested him to prepare the Catalogue of Palæozoic Plants in the Museum (1886).

For the great zeal with which he has pursued his researches, and the extensive additions to our knowledge which have resulted from his labours, the Council of the Society has deemed Mr Kidston well worthy to receive the Neill Prize.

The following Communications were read:—

1. On the Submarine Cable Problem, with Electromagnetic Induction. By the PRESIDENT.

2. Synthesis by means of Electrolysis—Part IV. Synthesis of Suberic

and *n*-Dicarbododecanic Acid. By Professor CRUM BROWN and Dr JAMES WALKER. *P.* xvii. 299 (*Abstract*); *T.* xxxvi. 211.

3. Graphic Records of Impact. By Prof. TAIT. *P.* xvii. 192 (*Abstract*); *T.* xxxvi.

4. On the Reduction of certain Algebraic Equations. By the Hon. Lord M'LAREN.

5. Preliminary Experiment on the Thermal Conductivity of Aluminium. By Professor A. CRICHTON MITCHELL, D.Sc. *P.* xvii. 300.

6. Pharmacology of Morphine and its Derivatives. By RALPH STOCKMAN, M.D., F.R.C.P.E., and D. B. DOTT, Esq., F.C.S., F.I.C. *P.* xvii. 321.

7. On the Fossil Flora of the Potteries' Coal-Field. By R. KIDSTON, Esq. *T.* xxxvi.

8. *Larix europæa* as a Breeding-Place for *Hylesinus piniperda.* By Dr W. SOMERVILLE. *P.* xvii. 255.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society:—

JOHNSTONE CHRISTIE WRIGHT.
T. D. BRODIE, W.S.
T. A. HELME, M.D.

Monday, 21st July 1890.

The Hon. Lord M'Laren, Vice-President, in the Chair.

The following Communications were read:—

1. On the Telluric Lines of the Solar Spectrum. By Dr L. BECKER. Communicated by Prof. COPELAND. *T.* xxxvi.

2. Some Points in the Physics of Golf. By Prof. TAIT.

3. On the Fishes of the Oil-Shales of Midlothian and Linlithgowshire. By Dr R. H. TRAQUAIR, F.R.S.

4. List of the Fossil Dipnoi and Ganoidei of Fife and the Lothians. By the Same. *P.* xvii. 385.

5. The Knot-Forms of the Eleventh Order. By Prof. LITTLE of Nebraska. Communicated by Prof. TAIT. *T.* xxxvi.

6. On the Electrodynamic Equations. By Prof. TAIT.

7. Note on Electrolytic Polarisation. By R. L. MOND, Esq. *P.* xvii. 302.

8. On the Interaction of Circular and Longitudinal Magnetisations. By Prof. KNOTT. *P.* xvii. 401.

9. Notes on the Ammonia and Organic Matter in Expired Air. By Dr HUNTER STEWART.

10. A new method for determining Phosphorus in Organic Phosphorus Compounds. By Prof. LETTS and R. F. BLAKE, Esq. *P.* xvii. 382.

11. Remarks on the work of the Session. By the CHAIRMAN. *P.* xvii. 406.

Donations from Authors to the Library of the Royal Society during 1890.

Adams (Prof. J. C.), M.A. I. On the Calculation of the Bernoullian Numbers from B_{32} to B_{62}. II. On the Mean Places of 84 Fundamental Stars, as derived from the places given in the Greenwich Catalogues for 1840 and 1845 when compared with those resulting from Bradley's Observations. 4to. Cambridge [1890].

Aikman (C. M.), M.A. Nitrogen: its Uses and Sources in Agriculture. 8vo. Glasgow [1890].

Bonaparte (Le Prince Roland). Le Premier Établissement des Néerlandais à Maurice. 4to. Paris, 1890.

——— Le Glacier de l'Aletsch et le Lac de Märjelen. 4to. Paris, 1889.

——— La Norvège et la Corse. 8vo. Genève, 1889.

Campbell (Arch. J.). Nests and Eggs of Australian Birds, embracing Papers on "Oology of Australian Birds" read before the Field Naturalists' Club of Victoria. 8vo. Melbourne, 1883.

Cayley (Arthur), Sc.D., F.R.S. Collected Mathematical Papers. Vol. III. 4to. Cambridge, 1890.

Congrès International de Bibliographie des Sciences Mathématiques, tenu à Paris du 16 au 19 Juillet, 1889. Procès-Verbal Sommaire. 8vo. Paris, 1889.—*From Le Ministère du Commerce, de l'Industrie et des Colonies.*

Dana (James D.), LL.D. Corals and Coral Islands. 3rd ed. 8vo. New York [1890].

Durham (Wm.), F.R.S.E. Evolution, Antiquity of Man, Bacteria, &c. 8vo. Edinburgh, 1890.

——— Astronomy, Sun, Moon, and Stars, &c. 8vo. Edinburgh, 1890.

Fletcher (L.), M.A. An Introduction to the Study of Minerals. 8vo. London, 1889.

——— An Introduction to the Study of Meteorites. 8vo. London, 1890.

——— Reprints from the *Mineralogical Magazine*, 1881–90.

Galileo. Le Opere di Galileo Galilei. Edizione Nazionale sotto gli auspicii di sua Maestà il Re d'Italia. Vol. I. 4to. Firenze, 1890. —*Presented by the Italian Government.*

Ganser (Anton). Die Wahrheit. Kurze Darlegung der letzten und wahren Weltprincipien. 8vo. Graz, 1890.

Gegenbaur (C.). Lehrbuch der Anatomie des Menschen. 4te verbesserte Auflage. 2 Bde. 8vo. Leipzig, 1890.

Goppelsroeder (Prof. Dr. Fred.). Ueber Feuerbestattung. 8vo. Mulhausen-i-Els, 1890.

Green (W. S.). Professor James Dana's "Characteristics of Volcanoes." 8vo. Honolulu, 1890.

Griffiths (Dr A. B.). Researches on Micro-Organisms. 8vo. London, 1891.

Haeckel (Ernst). Plankton-Studien. Vergleichende Untersuchungen über die Bedeutung und Zusammensetzung der Pelagischen Fauna und Flora. 8vo. Jena, 1890.

Harris (Wm. A.). A Technological Dictionary of Insurance Chemistry. 8vo. Liverpool, 1890.

Klossovsky (A.). Différentes formes des Grêlons observés au sud-ouest de la Russie. 8vo. Odessa, 1890.

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OBITUARY NOTICES.

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Professor Madvig. Born 1804, died 1886. By Professor Sellar.

Professor Johan Nicolai Madvig of Copenhagen was elected an Honorary Fellow of the Royal Society in the year 1878, on the ground of his eminence in Classical Philology. The Council of the Society could not have conferred the honour on a scholar more worthy to receive it. He may, without exaggeration, be pronounced the greatest Latin scholar born in the present century; and his services to Greek Philology alone would have made him eminent among Continental scholars, if his contributions to Latin criticism had not put them somewhat in the shade. In the devotion of a long life to the advancement of our knowledge of the great Greek and Roman writers, in the union of masculine vigour of understanding with great industry, and of singular acuteness with the soundest judgment, he recalls the type of some of the great scholars and grammarians of the Renaissance. The province of Classical Philology, in which he principally worked, was that of the criticism and emendation of texts, the most difficult of all the tasks which a classical scholar can set before himself, and one in which failure is more common than success, at least than such success as Madvig obtained. The two authors whom he made especially his own were Cicero and Livy; and his familiarity with, and insight into the diction of these, the two greatest masters of Latin style, have probably never been equalled in modern times. This familiarity and insight enabled him to produce his Latin Grammar, which during the third quarter of this century was the standard work on the subject; and which for clearness of method, logical coherence of thought, and simplicity of terminology, is not likely soon to be superseded in our higher schools and universities. Though he was chiefly eminent as a grammarian and verbal critic, yet in his

various editions of the *De Finibus* of Cicero, which appeared in 1839, 1869, and 1876, he gave a masterly exposition of the later Greek and Roman Philosophy; and, besides various treatises on such subjects as the "Equestrian Order," "Roman Colonies," the "Tribuni Aerarii," &c., one of the works of his later years was on the Roman Constitution, *Die Verfassung und Verwaltung des Römischen Staates.* But, undoubtedly, his especial distinction lay in his knowledge of MSS., his discernment of their relative value, and his clear and sound views of the true method and limits of conjectural emendation. On the subject of the causes and kinds of error found in ancient MSS., nothing better has been written than the introductory chapter to his *Adversaria Critica*, published in 1871. Of his emendations, it has been well said by an English scholar that "for brilliancy and inevitableness" it is difficult to find a parallel to them. This was due as much to his grasp of the right method of emendation, as to a rare union in his intellect of bold inventiveness and conservative sobriety of judgment.

He left a great name also as a teacher. All the younger generation of Danish scholars is said to have been trained on his method. But he was very far from being a mere scholar and teacher. He rendered important services to his country as a politician, and during the three years 1848–51, which were so critical all over the Continent, he was a member of the Danish Diet, and filled the office of Minister of Worship. After retiring from that office he became Inspector of the Higher Schools. In 1864, the year of the war between Denmark and Prussia and Austria, he wrote a pamphlet on the condition of affairs. During all his public career he was highly esteemed as an honest, sensible, moderate politician. He seems to have been a man of great geniality of nature; and his life, though one of continuous industry, never impaired by any bodily weakness till he became blind within two or three years of his death, was as far removed as possible from that of a pedant or recluse.

A short record of the more important events of his career, with their dates, is subjoined. He was born on the 7th August 1804, at Svaneke, in the island of Bornholm, an island in the Baltic lying between Denmark and Sweden, but forming part of the Danish kingdom. His father filled some subordinate official post, and was too poor to give his son a liberal education. The young Madvig

entered his father's office as a clerk at the age of ten, and continued in that capacity till his father's death in 1816. In later life Madvig believed that he had derived benefit from this early official training; and his own experience led him to think that boys would do better by not beginning to learn Latin till they were twelve years old. In the year 1817, when he was thirteen years of age, he was sent by some benevolent person who became interested in him to the *Staats Gymnasium* in Frederiksborg. He finished his University course in Copenhagen in 1825, and began his career as a teacher and examiner in Latin in the following year. In 1828, at the age of twenty-four, he was appointed Reader in Latin Philology, and in 1829 he was made Professor Extraordinarius of the Latin Language and Literature. In 1834 he collected and published, under the name of *Opuscula*, the introductory addresses which he was in the habit of delivering to his pupils, and a second series followed in 1842. His edition of Cicero *De Finibus*, which first appeared in 1839, and the appearance of his Latin Grammar in 1841, gained for him a European reputation as one of the foremost of Ciceronian critics ("Tullianorum criticorum princeps") and Latin grammarians. His Greek Syntax appeared in 1846. These two grammatical works were translated into German in 1844 and 1847, and soon after from the German into English; and those translations have passed through many editions. He had written a number of articles on the reform of higher schools between the years 1832–3, and in 1848 he was made Inspector of Education. In the same year he entered the Danish Diet as a representative for Bornholm, and was appointed a Member of the Ministry as "Cultus Minister." He remained in this office till December 1851, when he returned to the University as Professor, no longer merely of Latin, but of Classical Philology, which gave him the opportunity of lecturing on and emending Greek authors as well as Latin. At the same time he was appointed Inspector of Higher Schools. About this time he applied himself to what is perhaps his greatest work, his *Emendationes Livianæ*, which were first given to the world in a complete form in 1860. Between 1861–6 he brought out, in collaboration with his pupil Ussing, the well-known editor of Plautus, a complete edition of Livy. In 1860 and again in 1869, he travelled in Italy. In 1871 and 1873 appeared two volumes of *Adversaria Critica*,

containing the discussion already referred to on the right methods of emendation, and a number of emendations on Greek and Latin writers. It is to be remarked that his almost unerring faculty in emending Greek and Latin prose fails him, as it often does eminent Continental scholars, in his correction of the poets. It was no doubt the sense of their frequent failure, in attempting to restore the diction of the Latin and Greek poets, that made Niebuhr a strong advocate of the English cultivation of Latin and Greek verse composition at public schools. In 1884, a third volume of the *Adversaria*, or rather an Appendix to the two earlier volumes, was published, containing the results of his later readings of Homer, Herodotus, Athenæus, &c. The *Verfassung und Verwaltung des Römischen Staates* appeared in 1881–2. He continued to take part in political life till 1874, when he finally retired from the representation of his native island. But, though afflicted with blindness for some years before his death, he continued his philological activity till his final illness. He died on December 12, 1886.

Charles Edward Wilson, M.A., LL.D., *late H.M. Chief Inspector of Schools in Scotland.* By **William Jolly**, F.G.S., H.M. Inspector of Schools.

Charles Edward Wilson was born in Old Aberdeen, on the 31st of August 1815. He received a thorough education in the Grammar School of his native city, long distinguished for the success of its pupils; and in King's College there, entering it as second bursar, gaining a high place in classics, and leaving it with the degree of M.A. To extend his scholarship and experience, he resided for five years in Germany, chiefly at Frankfort-on-the-Maine, as tutor to a family of distinction. During his Continental stay, he studied the language, literature, and educational systems of Germany and France, and made acquirements which were of the highest utility in his after career. On his return to Scotland in 1845, he worked for some time in the Free Church Education Office, in Edinburgh. In 1848 he became Classical Master in Glasgow Academy, then under the rectorship of Dr James Cumming, late H.M. Inspector of Schools, with

whom he maintained unbroken intimacy. In February 1852 he was appointed Assistant Inspector, and for several years was associated with Dr Cumming in the examination of Free Church schools, during the days of denominational grants from the Privy Council. In 1859, having been placed in charge of the Western District of Scotland, he took up his head-quarters in Glasgow. His duties long involved constant travel over the whole of Scotland, including the Hebrides, experiences trying enough, especially before the present extension of steamboat and railway, but interesting and instructive, and thoroughly enjoyed; rewarded, moreover, by uncommon opportunities of insight into the educational, social, and religious condition of the people, and by enduring friendships. These wanderings inspired him also with a high appreciation of Scottish scenery, and made him an active pedestrian to the last.

Under the Scotch Education Act of 1872, Inspection became national, and the whole country was divided into separate districts, under independent officers, the late Dr John Gordon being Inspector-in-Chief, with residence in Edinburgh. After Dr Gordon's retirement in 1874, Dr Wilson succeeded him as metropolitan Inspector, and removed to the capital. On leaving Glasgow, he was presented with a handsome testimonial by teachers and friends, an expression of their high respect for him as a man and an inspector.

In 1860, Dr Wilson married a daughter of the late Dr Robertson, Commissioner to Her Majesty at Balmoral—a union of unalloyed happiness. They had two sons and one daughter, the eldest, a promising young officer in the army, having already served with distinction in Egypt and India.

As Chief Inspector, Dr Wilson had charge of the Edinburgh District, along with several assistants. In this capacity, he acted as general referee in disputed and difficult cases in Scotland; examined all the Scotch Normal Colleges, on which he yearly wrote special elaborate reports; and prepared the examination papers for the admission of candidates to these colleges and for teachers' certificates.

In religion, Dr Wilson was firmly attached to the Free Church, which he joined on his return from Germany, two years after the Disruption. He was an elder for four years in Kelvinside Free Church, Glasgow, and for twelve years in Free St George's, Edinburgh. He acted for many years as Representative Elder for

the Presbytery of Kincardine O'Neil in the General Assembly, which he regularly attended, though he took no part in the debates.

In politics, in spite of his natural Conservative instincts, he was a firm and consistent Liberal, quietly interested all his life in the progress of legislation, and regularly recording his vote for his party.

In study, he kept up his acquaintance with Classics, French, and German, setting the questions in these subjects for the Education Department for many years. He was much attracted by antiquarian researches, and became a Fellow of the Antiquarian Society in Edinburgh in 1870. Seven years later, he was elected a Fellow of our own Society.

Education, however, was naturally his chief study, and to this he devoted life-long attention. He edited for some months an educational journal issued by the Free Church shortly after the Disruption, and till 1852 acted as secretary to the Free Church Teachers' Association. He was a Fellow of the Educational Institute of Scotland from its foundation in 1847, though never, like Dr Cumming and others of his friends, its President—an honour which his official appointment no doubt prevented being bestowed upon him.

As Inspector of Schools, he was distinguished by uncommon equanimity, genuine kindliness, fair judgment, transparent honesty, quiet but real sympathy with teachers and pupils, and remarkable fidelity and care in the performance of duty. He was everywhere trusted, esteemed, and liked in all his official relations. To the Education Department, he was a singularly devoted servant, sacrificing his whole time to the work, and adorning the position. He richly deserved the encomium of the Privy Council in their Report for 1887, where they record their sense, certainly not too strongly, of "the serious loss sustained by the Department," through the death of one "who, during thirty-five years, had rendered most faithful, efficient, and valuable assistance to the cause of education."

In regard to Education, while he helped to make it effective and progressive by his annual examinations and Blue Book reports, his chief service was rendered in connection with the Training Colleges. Long earnestly interested in endeavours to raise the professional proficiency, scholarship, and general culture of teachers, he took an active part in combining the University with the Normal School course, by securing simultaneous attendance at both. This im-

portant subject was constantly referred to in his General Reports; and no one rejoiced more than Dr Wilson when this union was fostered by the special arrangements of the Department and the other authorities concerned, and when a yearly increasing number of Normal students began to attend the University and to take degrees—thus preparing, for the conduct of both our common and higher schools, the best class of teachers yet produced by the educational machinery of our country, in which Scotland is still far in advance of England.

In recognition of his official standing and long services to Education, he received, in 1870, the honorary degree of LL.D. from his own University of Aberdeen, for which he always retained a filial affection, while furthering general university advance.

Dr Wilson had no special social or intellectual ambitions. He took no active part in public affairs, and he has left no literary or other remains, beyond his numerous Blue Book reports. These were written with the extraordinary care which he bestowed on all he did, every sentence being touched and retouched till it satisfied his taste. They contain sound views and suggestions on education, and reliable estimates of the educational work with which he was brought into such close contact for more than thirty years.

Dr Wilson continued assiduously to discharge all his official duties almost to the very end. He passed quietly away in his pleasant home in Palmerston Place here, more suddenly than, from his robust health, had generally been anticipated, on the 18th of March 1888. He was interred in the Dean Cemetery, his funeral being attended by a large and representative assemblage of friends, colleagues, churchmen, and educationists.

Eminently endowed with the graces of character, Dr Wilson was emphatically the gentleman in mien, manner, and feeling, and exhibited much of what is now comparatively rare, the genial courtesy, if not courtliness, of an older time. "In his private capacity," says Dr Thomas Morrison, of the Glasgow Free Church Training College, one of his earliest and dearest friends—and his opinion will be generally endorsed by an unusually wide circle in all parts of the country—"Dr Wilson was a man greatly beloved. Full of varied information, genial and kindly in disposition and manner, simple as a little child, and retaining to the last wonderful freshness and

vitality, he was held in the deepest respect by all who had the pleasure and privilege of his acquaintance. A tried and trusted friend, a wise and sagacious counsellor, emphatically a good man, with no tinge of baseness in his composition, he has gone to his grave in a ripe and honoured old age."*

Robert Mackay Smith. By Professor Swan, LL.D.

Robert Mackay Smith was born in Glasgow in 1802, where he received his first schooling. Afterwards he was sent to the Moravian establishment at Fulnec; and finally he was for some time a student in the University of Glasgow. To the influences of Fulnec may be ascribed the lifelong interest he entertained for the Moravians; for whose missions in Greenland he did not fail to make liberal provision in his last will and testament.

Entering into business with his father Peter Smith, a merchant in Glasgow, he was in the habit of making journeys in Holland and Germany; and he thereby acquired a good knowledge of these countries. About the year 1834 he began business on his own account as a merchant and commission-agent in Leith, and always thereafter resided in Edinburgh, where he died on 4th April 1888; being then in his 86th year. From 1834 to 1878, when he finally retired, he maintained a prosperous business chiefly with Russia and Germany, while still retaining his old connection with Holland and other places. As might be inferred from his successful career, those of his friends best qualified to judge bear testimony to his unremitting diligence and excellent business talents. Not only did he take pleasure in business for its own sake, but the element of personal attachment entering largely into many of his business relations, his energies were often prompted by the desire to promote interests other than his own.

But while thus occupied, Mr Smith never failed to improve all available intervals of leisure in cultivating his literary and scientific tastes. In 1847 he became a life-member of the British Associa-

* *Educational News*, March 24, 1888.

tion; and, so long as strength permitted, he was a faithful attender on its meetings, from which he was rarely or never absent.

Geology was to him a subject of special interest; and, about 1857–8, when Iceland was much less readily accessible than it now is, he more than once visited that remarkable country. These visits to Iceland were doubtless the outcome of a genuine love of geological observation; and from that point of view, to his Icelandic experiences he always recurred with special delight. But this feeling was enhanced by other considerations of the kindliest personal character. While in Iceland, as was his wont, he had not failed to make many friends, with whom to the last he maintained a lively correspondence and intercourse. He used to furnish them with newspapers, periodicals, and scientific journals; and, it is said, he kept open house for Icelanders who might visit Edinburgh.

It was through Mr Smith's influence that the stations at Reykjavick and Stykkisholm were obtained for meteorological observations in connection with the Meteorological Society of Scotland. Among the various scientific associations with which he was connected there was none in which he took a livelier interest; and, as a member of its Council, he devoted much time and attention to its affairs. To this effect, the writer is indebted to Mr Buchan, its Secretary, for the following testimony:—

"At the time of his death Mr R. M. Smith had for twenty-three years been a member of Council of the Scottish Meteorological Society, and in that capacity had rendered most energetic and effective service to the Society in many ways and on many occasions. He took a strong personal interest in the extension of the Society's membership and sphere of operations. About the time when he became a member of Council, the Medico-Climatological Committee was formed; and, in establishing the stations in connection with the Committee, he gave invaluable assistance in securing for the Society the more important stations in Iceland, Färo, on the Mediterranean, and in South America. From the first, he was one of the most liberal subscribers publicly and privately to its foundation and maintenance; and he bequeathed to its funds a legacy of £500."

Among other Scientific and Literary Associations, of which Mr Smith was a member, may be named the Antiquarian Society of Scotland, and the Royal Scottish Society of Arts.

Of the Royal Society of Scotland he became a Fellow in 1855; and latterly might be reckoned as among its oldest members. For, as appears from the lists of Fellows at November 1887 and 1888, at the time of his death Mr Smith must have stood in order of seniority between 31st and 28th, out of a membership of about 470. This fact may be regarded as not unworthy of record, as illustrating the rapid change to which, at the hands of time, such a body as the Royal Society of Edinburgh is subject. Although Mr Smith never contributed in authorship to the *Transactions* of our Society, he was one of its most loyal members, who always took the deepest interest in its welfare; and who, by his reversionary legacy of £1000 to its funds, may be numbered among its most munificent benefactors. Our Librarian, Mr Gordon, bears testimony that "in his later days, when no longer able to attend its evening meetings, Mr Smith rarely allowed a day to pass without an afternoon visit to the Society's Rooms."

Mr Smith was appointed a member of the Board of Visitors of the Royal Observatory of Edinburgh about 1855. He continued to hold that post during the remainder of his days; and, as Professor Piazzi Smyth has written, "for so long attended, whatever meetings were held, more regularly than any other member." He always took a lively interest in the welfare of the Observatory, and in many ways sought to promote its usefulness. Entirely at his own cost, he erected an electrically controlled clock in the Edinburgh University Buildings, and also two other clocks, similarly electrically controlled from the Observatory, at the entrances of the Leith Docks. The dropping of the Observatory Time-Ball on the Calton Hill being too often invisible at Leith owing to the smoke of the city or to fogs, Mr Smith's purpose in erecting these clocks was the better to enable shipmasters to obtain the rates and errors of their chronometers; and it is just cause for regret that, for want of co-operation in the shape of needed supervision and care-taking, his reasonable expectations ultimately failed to be realised.

Throughout his long life an earnest supporter of schemes devoted to the welfare of his fellow-creatures, Mr Smith gave liberally to the Royal Infirmary of Edinburgh, the Children's Hospital, the Dalry Girl's Reformatory, and other benevolent institutions. With a wholesome contempt for mere conventional almsgiving, he never

shrank from any obloquy which might be incurred by saying no to applicants for schemes regarding whose utility he was not satisfied —wisely husbanding his means for the unostentatious relief of real want. It will serve well to illustrate his ways and principles of giving, if reference here be made to a bequest of £500, which he left in charge of his friend Mr Thomas Davidson, who writes:— "Mr Smith used often to give me sums of money to expend on material comforts, or more correctly, essentials for poor children; and used to listen with evident interest to my stories about their quick impulsive gratitude and their unconscious humour in its expression. I am sure, if we knew it fully, there were many other ways by which he gave charity, for it was his habit not to boast about it to any one else. That special legacy, however, is a concrete example of what was, I believe, a mood habitual to him, in his last years at any rate, and will help to complete the picture of the man to those who did not know his inner life. The terms of the legacy were (after some words of kindness about myself which I shall not soon forget) 'for the poor children of the streets of Edinburgh, to be spent by him as he thinks best for their good.'"

Among associations for the intellectual improvement of his fellow-townsmen, the Philosophical Institution of Edinburgh received a very large share of his time and energies. Elected in 1851, he continued on the Board of its Directors until the day of his death; and even in the very last year of his life he greatly interested himself in the formation of a Reference Library for the use of the lady members, to which he presented many valuable books of reference and of general literature.

Towards the erection of the new Medical Buildings of the University of Edinburgh he afforded material help; and he presented £2000 for the foundation of bursaries, one in Natural Philosophy, and one in Chemistry, to be held in the Universities of Edinburgh and Glasgow alternately.

Mr Smith never married. His eldest sister had kept house with him throughout his residence in Edinburgh; and, on the death of his father, his mother Euphemia Mackay came also to live with him. Mrs Smith, to the end of her long life, retained her faculties in a wonderful measure; and her memory for dates, names, and connections remained to the last apparently unimpaired. She was a

native of Sutherlandshire, related to the Mackays of Bighouse; and she could trace her descent from Donald, first Lord Reay, an ancestor whose exploits she was fond of relating. With the ladies of the Reay family, until the final extinction of its Scottish branch, she kept up a friendship and correspondence. At length, about 1878, she died at the great age of ninety-six, and was in a few years followed by her daughter.

In his house, 4 Bellevue Crescent, for the best part of forty years, Mr Smith had been in the habit of receiving and entertaining his friends, with a kindness which many yet living must remember, and still more who have passed away had experienced. But although now so far advanced in years, and no longer supported by the companionship and help of his kind and able sister Euphemia, Mr Smith's hospitable dispositions maintained their supremacy to the end. In illustration of this notable trait of his character, and not less of the *esprit de corps* which he always so strongly manifested for the literary or scientific associations with which he was connected, the writer of this notice desires to record that when in 1882, although non-resident in Edinburgh, he had accepted the Presidentship of the Royal Scottish Society of Arts, Mr Smith, so soon as he learned this, said—"There is your room, it shall be ready for you whenever you come to attend the meetings of the Society, and you will live with me." Thus in its latest days an old friendship became more than ever intimate; and, for a period of nearly three years, the writer had frequent opportunities of becoming better acquainted with the many good qualities which distinguished Mr Smith. Very notable was the youthful freshness of his mind. His childhood had been spent in Hope Park near Glasgow, then a pleasant suburban residence, where, as he used to say, pears and apples ripened the like of which were no longer to be found, but which has now entirely disappeared before the overwhelming flood of town and railway extension. His love of nature, thus early acquired, he retained even in advanced old age: that "Eye among the blind" for beholding beauty in landscapes and in flowers, and that well-marked trait of sympathetic natures, fondness for animals, keen observation of their *personal* qualities, and hearty enjoyment of their amusing ways. To this last his strong sense of humour doubtless contributed. No one more enjoyed a

good story; and his quiet but genuinely hearty laugh his friends must well remember.

Mr Smith was an ardent lover of works of art, more especially of pictures, which he collected with discrimination. Not a few of these he gave away to friends, and some of the more important he presented to the National Gallery of Scotland. From 1865 to the time of his death he was a member of the Committee of Management of the Royal Association for the Promotion of the Fine Arts in Scotland; and Mr H. W. Cornillon, its Secretary, testifies to the excellent services which he rendered in that capacity; and that, while never forgetful of the interests of the Association or of its contributors, Mr Smith was unremitting in his exertions in behalf of young artists of merit by directing the attention of the Committee to their works.

It has elsewhere been justly remarked regarding Mr Smith, that "no one took up more firmly and persistently a cause which he believed to be sound and right." Of this, his painstaking exertions in conjunction with his friend Lieutenant-General Sir James E. Alexander, C.B., in calling the attention of Government to the Defences of the Harbours of Scotland, and which may be regarded as having borne fruit in the erection of the fortifications on Inchkeith and Kinghorn Ness, may serve as an instance. Thus too, when, as must indeed be admitted, he sometimes took very decided and it may be peculiar views, in which his friends were unable to agree with him, he nevertheless was wont to adhere to them with characteristic determination.

But those who knew him best could never doubt the sincerity of his convictions, nor, in the occasional manifestations of the severer aspect of his character, could they lose sight of its essential goodness, or forget its habitual geniality. Thus also it proved that, in spite of his characteristic peculiarities, Mr Smith's name deservedly acquired and to the end maintained in commercial circles the highest respect.

In his day he was Chairman of the Chamber of Commerce, and a Director of the Commercial Bank of Scotland. Although never appearing prominently as a citizen of Edinburgh, his means and opportunities were for him continually a "talent well employed," whose influence for good may not soon be forgotten.

The late Robert Gray, Vice-President. By R. H. Traquair, M.D., F.R.S.

The brotherhood of science includes the amateur as well as the professional, and the amateur who devotes what energy and time he can snatch from a laborious business career to the furtherance of human knowledge, whether by original research, by aiding the researches of others, or by keeping up an interest in science among his friends and fellow-citizens, is worthy of being held in affectionate remembrance, even by those who are able to devote their entire time and their whole minds to scientific work. And at a time too, when so many *litterateurs* boldly and ignorantly express their abhorrence of the entire subject of Biological Science, our sympathies are especially enlisted in favour of those who, not as so-called popular writers, but as steady and devoted workers, add their own portion to our knowledge of particular branches, and also by their example, their conversation, and their friendship diffuse a liking for such studies in the minds of others.

An amateur in the best sense of the word was Robert Gray, the subject of the present short memoir. He was born at Dunbar in 1825, the son of Archibald Gray, merchant in that place, and a man of considerable literary tastes, but to whom it was not permitted to have any great share in the direction of his son's career, as he died when the latter was only eight years of age. Two years afterwards his mother followed.

It was also at Dunbar that young Gray received his school education, and his master, Mr Lyon, is known to have entertained a high respect for the promising abilities of his pupil.

His career as a man of business may very briefly be sketched. At the age of fourteen he entered the branch of the British Linen Company Bank at Dunbar, in which he served as apprentice for four years. He then, at the age of eighteen, entered the service of the City of Glasgow Bank, and two years afterwards left Dunbar, on obtaining an appointment at the head-office of the same bank in Glasgow.

In Glasgow he served successively in his bank as secretary's clerk, teller, and inspector of branches, finally becoming agent at the St

Vincent Street Branch, where he remained until, in March 1874, he removed to Edinburgh to take up the office of Inspector of Branches in the Bank of Scotland. Eight years afterwards he received his last promotion, namely, to the office of cashier in the same bank, for only five years afterwards, in 1887, the post was again vacant through his lamented death. The steadiness of his promotion, and the popularity and respect in which he was always held both by his superiors and inferiors in official rank, are ample evidence that, notwithstanding his other tastes, Robert Gray was not slothful in business.

Mr Gray formed no exception to the rule, that a taste for natural history usually displays itself at an early age. As a boy he commenced to form a collection of natural history specimens, and, as is also usually the case, his first studies were not confined to any particular speciality, but commencing with birds' eggs, he went on to birds' skins, which he taught himself to prepare, and soon he also included fishes, shells, crustacea, and insects, in the attic which served him as a museum. We have also the usual complaints about his bringing "stinking beasts" into the house, made by the lady-housekeeper, who managed the domestic affairs of the family after the early death of his parents.

Gradually, however, his attention became concentrated on the subject of ornithology, and by his appointment to the post of inspector of branches of the City of Glasgow Bank, his opportunities for study in this direction were much increased. He had now frequent opportunities of travelling, especially in the western parts of Scotland, and the method which he systematically pursued on those journeys was, first, of course, to settle his business with the agent of the branch in the place he was visiting, and then to inquire whether there was any bird-stuffer in the place, or any other person who was interested in the study of birds. In this way he gained many ornithological friends throughout the country, acquired many specimens of birds, filled many a note-book with notes and sketches; and to those journeys the work by which Mr Gray is best known—the *Birds of the West of Scotland*—owes its origin. This delightful book, published in 1871, is now long out of print, and copies at present fetch five or six times the original published price.

During the last few years of his life Mr Gray was engaged, in conjunction with his friend Mr William Evans, in collecting material

for a companion book on the *Birds of the East of Scotland.* The appearance of this work has been eagerly looked forward to by British ornithologists, and all will be glad to know that Mr Evans intends to complete and to publish it with as little further delay as possible.

Mr Gray was one of a small number of friends of congenial tastes, who in 1851 founded the Natural History Society of Glasgow, a Society of which he became secretary about the year 1860. Under his guiding influence, the Society increased in numbers and prosperity, and he himself contributed many papers to its *Proceedings*, but on his taking up the post of agent of the St Vincent Street Branch of his bank, he felt constrained to resign the secretaryship, as incompatible with the duties of his new office.

Two years afterwards, on his removal to Edinburgh, he entered on a new sphere of usefulness in connection with Society work. Having joined both the Royal and the Royal Physical Societies of Edinburgh, he soon was elected first a Councillor and then a Vice-President in this, the former Society, and the interest which he took in the entire work of the Society, as well as his success as a chairman at the evening meetings, are in the remembrance of us all. But it is more especially with regard to the Royal Physical Society that his influence for good made itself felt. This old Society,—the oldest scientific society in Edinburgh with the exception of the Royal Medical,—in whose meetings men like Fleming, Hugh Miller, Goodsir, and Wyville-Thomson had taken an active part, a Society with which was incorporated the celebrated Wernerian, and whose special function in the encouragement of scientific work cannot fail to be recognised, had fallen into one of its periodic fits of depression. In 1877 the secretaryship became vacant, and fortunately for the Society and its work in Edinburgh, there was now no official objection to his undertaking the work. Accordingly he was elected secretary, to the inexpressible satisfaction of all those who wished the Society well, and, in fact, the effect upon its prosperity was magical. He speedily gathered around him a knot of energetic friends determined to raise the Society again to its proper position, new rules were drafted, passed, and put in force, the meetings became more largely attended, papers of a higher class than formerly came in, and every year saw the publication of a still handsomer fasciculus of *Proceedings*.

This infusion of new life into the Royal Physical Society was entirely due to Mr Gray, and affords admirable illustration of some of those personal qualities which distinguished him as an individual. No doubt he could have effected little by himself, without the aid of those friends who zealously worked with him, but they were drawn together by his personality, in which seemed to be concentrated the qualities desirable in the secretary of a Society of the kind.

For he was, in the first place, a good man of business, and the Royal Physical Society was at the time sorely in need of such a one to guide it through its troubles, financial and otherwise. Then he was not only eminent in one branch of natural history which he had adopted as a speciality, but his general scientific sympathies were extensive. But it was more especially his human sympathies which gained for him the willing co-operation of the older members of the Society, and excited the interest of the younger, for the love and respect which he diffused around him acted as a powerful incentive to others towards aiding him in furthering the objects which he had at heart. Mr Gray was one of those very few men who are loved by all who know them. It unfortunately falls to the lot of most people to know that though they may have on the one side those who look on them with sympathy and approbation, yet on the other stand those who regard them with jealousy and dislike. But Mr Gray's warm and open-hearted geniality, his sterling integrity of purpose, his equable temper, and his gentle affability, disarmed the hostility of those who might have been opposed to him, and left him, as a friend of mine has remarked—"a man with many friends and no enemies."

And if Mr Gray's personal qualities attracted towards him the love and admiration of those who knew him, so also did he return the affection of his friends with overflowing interest. He was a genuine Lowland Scotsman in his faithful and true-hearted nature, and his amiable tendency was rather to magnify the virtues and deserts of his friends, than to criticise or think lightly of them for their faults and their failings.

During the last five years of his life Mr Gray had one or two severe attacks of illness, and his relatives and friends began to observe with sorrow and concern that his bodily health was no longer what it had been in earlier days. Nevertheless, on the very morning of

the day on which the fatal blow descended, he seemed to the members of his family to be in better spirits than usual. On the 16th February 1887, he was found unconscious in an apartment of his house used by one of his daughters as a studio for painting, and on the morning of the 18th he breathed his last, and so passed from amongst us one whose place in the scientific circles of our city, and in the affections of all who knew him, may never again be filled.

Asa Gray. Born 1810, died 1888. By Professor Frederick O. Bower.

It rarely happens that the death of a prominent man leaves the world with an unanimous opinion as to his merits: yet such would seem to be the case on the death of Asa Gray; the familiar stricture "de mortuis nil nisi bonum" appears to be here unnecessary, for all agree in according to him the highest position, not only as a scientist, but also as a man. It was the good fortune of many men of science in this country to have been acquainted with him, and that especially at the time of his last visit to Europe in 1887: none could fail to be struck with his combined geniality and simplicity of manner, while those who knew his work recognised that the placid surface covered the greatest depths of intellect.

He was born in 1810, of a family settled in Sauquoit Valley in the far west, and devoted to rural rather than intellectual pursuits. His father, soon recognising the bent of his son's mind, gave him such opportunity of schooling as the country afforded, and subsequently young Gray entered on the study of medicine. Though he graduated as doctor, he never practised, but following his natural inclination for purely scientific work, which had already declared itself, he became in 1831 instructor in Chemistry, Mineralogy, and Botany, and performed such duties in various colleges and schools. In 1833 his appointment as assistant to Professor Torrey, in New York, marked an important era in his progress. His interest was at first chiefly mineralogical, and it was in this subject that he first appeared as a writer; but during his period of work with Torrey, his attention was diverted more exclusively to botany, while the literary influences which then surrounded him may be recognised as conducing to that excellence of style which subsequently distinguished

him as a writer. In 1838 he accepted the chair of Botany in the University of Michigan, with the stipulation that he should be allowed first to spend one year abroad: this led to the first of his European journeys, of which the last is so fresh in the memory of many, having been made in 1887, and completed only four months before his death. On his return to America after his first journey to Europe he never actually lectured at Michigan, but was appointed to the chair of Natural History in Harvard College, and went into residence in Cambridge in 1842. Here he lived and died, exercising for a period of forty-six years an almost creative influence upon American botany. It is true others had worked before, but it was chiefly owing to Gray that a common understanding was thoroughly established between the systematists of Europe and those of America. During his tenure of office the exploration of the flora of North America was pushed rapidly forward: not only did he receive and work up the collections of others, but he also travelled and collected himself. His private herbarium gradually grew till in 1864, he offered it, together with his library, to the College, on condition that it should be placed in a suitable building: this condition was fulfilled, and his herbarium of 200,000 specimens, and his library of about 2200 works, then became the foundation of what is now the most representative botanical institution of America. To have brought together and arranged such an herbarium, and to have organised a botanic garden, would have been a sufficient occupation for most men; but Dr Gray was also engaged in constant lecturing up to 1872, and in producing such a volume of literary work as few could equal. As the result of his teaching, he has left behind him a whole generation of capable botanists, who not only follow in his special line, but have differentiated in their branches of study, so that the whole area of the science is now fitly represented in his University of Harvard. Those who knew him personally can well imagine how firm a hold his winning manner and ready sympathy would take upon the minds of his students, and this is one of the first elements of success in teaching. But not only are the wide interests of Dr Gray to be traced in the fact that he left behind him among his pupils men who followed other lines in the science than his own specialty; it is patent from his writings, and those who look over the comprehensive list of his

published works will feel that though his greatest contributions were to geographical distribution, classification, and morphology, he was no mere specialist in these lines. He was one of those who quickly pick up the spirit of the time in which they live; in no matter was this more conspicuous than in his view of the Darwinian theory, when it was first brought forward. He took it up at once, and was one of its first prominent adherents: his essays on the subject, now collected in the volume entitled *Darwiniana*, will perhaps be his work best known to the general public: written by one who professes to be an acceptor of "the creed commonly called the Nicene," they will go far to resolve those religious difficulties which have obstructed the progress of Darwinism. At the same time, the main points of the theory of evolution are stated in these essays with a precision and appreciation too often wanting in articles designed for the general public, while fresh illustrations were supplied from the rich stores of facts at the disposal of their author. Doubtless he himself will have regarded these contributions as a bye-play in the main work of his life, which was to produce a "Synoptical Flora" of North America: after thirty-five years of preparation for this great undertaking, the first part was issued in 1878, the second in 1884: he did not live to complete it, though the work was left in such a position that it can be taken up and finished by others. His influence as a teacher upon the botany of his time was extended beyond his own lecture theatre by means of his text-books; his *Elements of Botany* appeared as early as 1836, and this book formed the foundation of his *Botanical Text-Book* of 1842, which ran through numerous editions. In accordance with the progress of the time, this work has outgrown its original form, and in its latest editions was planned to consist of four volumes; of these that on morphology was by Gray. It is a book too well known in this country to require any description here; in itself it is a sufficient evidence of the author's didactic power.

It would be impossible here to do more than mention the most important of Gray's writings. Through a period of over fifty years his prolific mind was continually producing fresh work, original monographs, text-books, works on classification and distribution, reviews, obituary notices: the very list of them covers over forty closely printed pages. He lived a strenuous, active life, inspiring with his

own energy and thoroughness those with whom he came in contact. He almost died in harness, after an illness of only short duration; he had, however, the satisfaction of knowing that his work was appreciated by the men of his time, for on his last visit to Europe the highest academic honours were showered upon him, as a due recognition of the services of America's greatest botanist.

Dr Samuel Drew. By Arthur Jackson, Esq., M.R.C.S.

Dr Samuel Drew of Chapeltown, near Sheffield, was an excellent type of a body of men, who in the past have done good work for the country, but who are now, we grieve to say, rapidly disappearing. The well-educated practitioner in the country, who is ever ready to take up with new ideas and with fresh modes of thought, who perseveres to the end of his life with his education, has had great opportunities of conferring benefits upon those around him beyond the ordinary work of his profession, and has in the past wielded an influence in his district, second only to the vicar of the parish. Such was the position occupied by Dr Drew in Chapeltown.

Educated in Edinburgh and Paris, his clear intellect enabled him at once to perceive the advantages of being a student in those far-famed centres of education; and there he laid the foundation of his knowledge of medicine and science, which added to the happiness of his life in after years, and which was of such advantage to his neighbours.

He took the customary degrees in Edinburgh and London in 1852; proceeding to take the M.D. of Aberdeen in 1859; the B.Sc. and D.Sc. of Edinburgh in 1875 and 1877; and the Cambridge Certificate in Sanitary Science in 1875.

He was a conscientious and able practitioner, commanding the respect and confidence of his patients without having recourse to those artificial aids which are so common at the present time. He identified himself with the public work in his district, enjoying in his spare moments his well-chosen library, and contributing to the pleasure and benefit of his fellow medical men in Sheffield, by the part he took in the meetings of the Medical Society, of which he was in 1878–9 the President.

A clear thinker and a good talker, a well-informed and a con-

scientious man, his memory will long be revered by those who had the privilege of enjoying his friendship.

William Dickson. By Josiah Livingston, Esq.

William Dickson was born in Edinburgh in 1817, being the son of a very much respected merchant.

After going through with distinction the High School and the Arts course at the University, he was offered a position in the Commercial Bank of Scotland, where he acquired much business experience, and gained the respect and esteem of those in the management—one of the juniors in the establishment being Sir William Fettes Douglas, *P*.R.S.A.

Mr Dickson found it his duty, after several years' service in the Bank, to come to the assistance of his father in the extensive paper business which he carried on, and, along with his younger brother David, he joined the firm of James Dickson & Co., in which the talents and energies of the two brothers were eminently successful.

Diligence in business did not, however, prevent him from engaging in other studies, and he showed by his works in oil and water colours that his pencil wanted only practice to place him in a high position among artists.

In 1852 Mr Dickson made a lengthened tour in Syria and Palestine, districts then comparatively unknown, and on his return his descriptive lectures were eagerly sought, and gave much instruction and profit and delight to many and crowded audiences throughout Scotland. He brought from his journeyings the materials for the formation of a museum descriptive of the Holy Land, its products, and the manners and customs of the inhabitants, which collection is of very considerable value, illustrating in a most attractive manner the Holy Scriptures.

Mr Dickson was elected a Fellow in 1869, and he took much interest in the proceedings of the Royal Society, although modesty prevented him from any active part in its discussions.

He was an enthusiast in the cause of education, to which for upwards of forty years he devoted his enlightened talent and unwearied energies.

He died on the 15th April 1889.

Dr Andrew Graham, R.N. By John Romanes, S.S.C.

(Read January 20, 1890.)

Another of our Fellows has gone to his rest. Dr Andrew Graham, Fleet Surgeon, Retired, died at his residence at the Albany, Piccadilly, London, on the 1st December 1889, in the 73rd year of his age. He was also a Fellow of the Royal Geographical Society, London.

He came of a family of medical practitioners. His grandfather, Dr Andrew Graham, settled in Dalkeith upwards of a century ago, and after some forty years of extensive practice, died in 1824, leaving five sons, four of whom were graduates in medicine in the Edinburgh School. The eldest of these, Dr Walter Graham, the father of the subject of the present notice, assisted his father for many years, and died of fever contracted in practice at Dalkeith in 1827, while the subject of our notice was but a boy of about ten years of age. After passing the earlier stages of his education, the latter prosecuted his studies at the University of Edinburgh. At the medical classes he was a diligent and successful student, and in 1837 became a licentiate of the Royal College of Surgeons. In the following year he graduated as doctor of medicine; and shortly thereafter, on the 19th November 1838, he entered the service of his Queen and country as an assistant surgeon in the Royal Navy. He became staff surgeon on 27th July 1847, and fleet surgeon on 22nd June 1864; and, after a full period of service, retired (with retired pay) on 1st April 1870.

He served for some time in the Mediterranean and East Indies; and during the Russian war he served as surgeon on board the flag ship in the Baltic, under Admiral Sir Charles Napier. At a later date he served in the West Indies in H.M.S. "Agamemnon," under Captain Thomas Hope, Pinkie.

He received the Baltic medal, and also Sir Gilbert Blane's gold medal.

Wherever he served, he proved himself an able and accomplished navy surgeon, admirably cool in danger, and attentive, both in times of peace and war, to the health and welfare of those under his care.

He was of a quiet unassuming disposition, and endeared himself by his goodness of heart to all who were acquainted with him. He died a bachelor; but though many of his friends had gone before, he is survived by many—friends, relatives, brother officers, and fellows—who will sincerely mourn his loss.

Sir James Falshaw, Bart. By **Bailie J. A. Russell, M.A.,** M.B.

(Read January 6, 1890.)

Last June Sir James Falshaw, Bart., J.P., D.L., who had been long and honourably known in connection with railway and municipal matters in Scotland, died in Edinburgh, at the age of 79. He was the son of a wool merchant in Leeds, where he was born on 21st March 1810, the sixth of a family of fourteen; but it was in Scotland that he won fortune and reputation, and that he finally settled. At school he sat on the same bench with Sir John Hawkshaw under Mr Jonathan Lockwood, and at the age of fourteen he was articled for a seven years' apprenticeship to Mr Cusworth, architect and surveyor. At this time he laid the foundation of his first success by mastering the subject of skew arches. He then became agent in charge of a section of the Leeds and Selby Railway for the contractors Messrs Hamar & Pratt, who subsequently appointed him to the entire charge of the construction of the Whitby and Pickering Railway. In this bit of work he gained experience of steep gradients, curves, and other difficulties which afterwards stood him in good stead. Thereafter he obtained the position of chief-assistant to Mr G. Leather, engineer of the Aire and Calder Navigation, Goole Docks, &c. During the seven years he was with Mr Leather he had a share in preparing many important engineering schemes, among which were the Leeds Waterworks, involving a tunnel of 1¼ mile, the Bradford Waterworks, and the Stockton and Hartlepool Railway, of which Mr (now Sir John) Fowler was resident engineeer. When 33 years of age, just at the time when the great outburst of railway construction was in progress, he began business on his own account as a railway engineer and contractor, and achieved considerable success. He then joined the staff of Messrs John Stephenson & Co. in the making of the Lancaster and Carlisle Railway, which now forms part of the London and North-Western Trunk Line. This engagement brought him into contact with Mr Brassey and Mr Mackenzie, who were associated with Mr Stephenson, and with the first named well-known engineer he enjoyed a life long friendship. Under the auspices of this firm, Sir James Falshaw took a leading part in making large portions of the Caledonian and Scottish Central Railways and Scottish Midland ailways. In 1851 his

connection with the firm of Messrs John Stephenson & Co. ceased, and Mr Falshaw joined Mr Brassey as contractors for the construction of the Inverness and Nairn and Elgin Railway, Mr Falshaw taking the entire management. By himself he contracted for the upkeep for seven years of the Scottish Central and Scottish Midland Lines, and for the construction of the Denny branch Scottish Central Railway, and the Portpatrick, Stranraer, and Glenluce Railway. With Messrs Morkill & Prodham, two former assistants, he contracted for the Berwickshire Railway and the Blaydon and Conside Branch of the North-Eastern.

He became a director of various minor railways, manufacturing, banking, shipping, and insurance companies, but never forgot the duty which he owed, to devote a portion of his time to the public good, and took the opportunity of a four years' residence in Nairn to enter the Town Council, whereupon he was elected Senior Bailie. In 1858 Sir James Falshaw settled in Edinburgh, and at once began to interest himself in the affairs of the city. He was returned to the Town Council in 1861, and three years later he was elected a Bailie, and on the resignation of Mr James Cowan to become Member of Parliament for the city in 1874, he was elected Lord Provost. At this time the Town Council had to face many difficult questions, including the promotion of no less than three bills in Parliament; and the city of Edinburgh owes much to the energy and sagacity shown by Sir James Falshaw in all departments of the city's work. He showed great zeal in making preparations when cholera threatened to visit the town, and he had a considerable share in passing the Improvement Trust Act, which has done so much for the health and amenity of Edinburgh. Among the improvements executed during his reign as Lord Provost may be mentioned the widening of Princes Street, the widening of the North Bridge, the opening of West Princes Street Gardens to the public, the covering in of the Waverley Market, and the purchasing of the Arboretum. Undoubtedly, the most important work of Sir James Falshaw's municipal life was the introduction of the Moorfoot water supply into Edinburgh—a scheme in which he took a deep interest, and which was completed after his term of office as Lord Provost had expired, but while he was Chairman of the Works Committee of the Water Trust. His Baronetcy was conferred upon him in 1876,

on the occasion of the visit of the Queen to inaugurate the equestrian monument erected in Charlotte Square to the memory of the Prince Consort. Sir James also filled the honourable office of Master of the Merchant Company. He was long connected with the North British Railway Company, of which he was elected Deputy-Chairman in 1881, and subsequently Chairman, an office which he held until 1887, when advancing age led him to vacate the more onerous position to again become Deputy-Chairman. It was during his tenure of the chair that the Tay Bridge was opened, and he had a lively interest in the still greater undertaking promoted by the Forth Bridge Company, of which he was the first Chairman.

Sir James Falshaw was elected an Associate of the Institution of Civil Engineers in 1854, and became a Fellow of this Society in 1866.

He was twice married—first, in 1841, to a daughter of the late Mr Thomas Morkell of Astley, who died in 1864; and again in 1871 to a daughter of Mr Thomas Gibbs, Norwood. Sir James was predeceased by Lady Falshaw, and left no family. He was not only recognised as a man of sterling integrity, but one of high Christian character, and though of a brusque demeanour, he had many friends. He was a Wesleyan Methodist, and in politics a Conservative. In the conduct of public business he was clear-sighted and hard-headed, utterly fearless, and full of energy and determination, and the results of his reign—both at the Town Council and Railway Board—were generally excellent, though it must be confessed, in the words of the *Scotsman*, that his impatience of long speeches and his laconic methods of conducting business, occasionally staggered the advocates of liberty of speech. In gratitude for his services to the city, his portrait was painted by subscription among leading citizens, and now hangs in the Council Chambers.

Dr Edmund Ronalds. By J. Y. Buchanan, Esq.

Dr Edmund Ronalds was born in Canonbury Square, London, in the year 1819. His father was Edmund Ronalds, merchant in London, and his mother Eliza Anderson, daughter of James Anderson, LL.D., also of London. His father's elder brother was Sir Francis

Ronalds, well known in connection with the origin of the electric telegraph. He was educated at a private school in England, after which he went abroad, and studied at Giessen, Jena, Berlin, Heidelberg, Zurich, and Paris. At Giessen he was the fellow-student of Hermann Kopp, Fresenius, Will, and other well-known chemists. He returned to England in 1840, and lectured on chemistry at St Mary's and the Middlesex Hospital. In 1849 he was appointed professor of chemistry in Queen's College, Galway. In 1856 he gave up his professorship, and took over the Bonnington Chemical Works, where the tar and ammonia liquor of the Edinburgh Gas Works were dealt with. At the expiry of the contract Dr Ronalds retired from business, and lived at Bonnington House until his death, on 9th September 1889. He was a constant attendant at the meetings of the Society, and although he rarely took an active part in its proceedings he always took a lively interest in everything that went on. He had an admirably appointed laboratory, with the use of which he was most generous; and among the numerous chemists who, either as students or teachers, have from time to time resided in Edinburgh during the last thirty years, there are none who do not remember him with affection.

Joseph J. Coleman. By Professor M'Kendrick.

Joseph J. Coleman died on 18th December 1888, in the 49th year of his age. Trained first as a chemist and druggist, he was early led to the study of chemical science, and so soon as in his 22nd year he contributed papers on chemical subjects to the *Proceedings* of the British Association. In course of time he became chemist to the Young's Paraffin Light and Mineral Oil Company, and in this capacity made original investigations on the gases produced in the distillation of bituminous shale. By submitting these to great pressure, at a low temperature, Mr Coleman obtained highly volatile liquid hydrocarbons. This investigation led him to the problem of the mechanical production of low temperatures, and to the invention of the well-known machine by which a low temperature is maintained in the holds of steamers conveying large cargoes of fresh meat from America and Australia. Along with Mr James Bell, the method was successfully carried out,

and Mr Coleman's dry-air mechanical refrigerators were fitted up in many steamers. Mr Coleman acquired a modest fortune from his invention, and, retiring to Bearsden, near Glasgow, he built a small private laboratory in connection with his house, and devoted himself entirely to original investigation. He contributed numerous papers to the Philosophical Society of Glasgow, to this Society, to the Society of Chemical Industry, and to the Institution of Civil Engineers. For many years Mr Coleman suffered from weak health, and at length his frail body succumbed to a complication of disorders. He was a man of bright and lively intelligence, who took an original view of any scientific question to which his mind was directed. Although eminently practical as a chemical engineer, he had a great regard for the first principles of science, and even for those problems in chemistry and physics that are of merely speculative interest to most men. Few were more gifted with the power of recognising the practical applications of scientific theory, and it was this quality of mind that led him to the invention of the machinery for the mechanical transference of heat with which his name will always be associated.

Franz Cornelius Donders. By Professor M'Kendrick.

This distinguished physiologist and ophthalmologist was born in North Brabant on 27th May 1818, and died at Utrecht on 24th March 1889. Educated in the Dutch Royal Hospital for Military Medicine and Surgery, he practised for a time as army surgeon in Vliessingen and in the Hague; but an anatomical and pathological investigation on the nervous centres having attracted the attention of the authorities, he was soon appointed lecturer on anatomy and physiology to the Royal Military Academy in Leyden. This office he held till 1848, when he was appointed professor extraordinary in the Medical Faculty of the University of Utrecht; in 1852 he became an ordinary professor; and on the death of Schroeder van der Kolk, in 1862, he was called to the chair of physiology. He filled this chair till 1889, when he retired in compliance with the law of the universities in Holland, by which no professor can occupy a chair after attaining his seventieth year. Soon after his retirement his health gave way, and he died after a series of apoplectic attacks.

In 1843, when physiologists were much occupied with the cell theory of Schleiden and Schwann, Donders carried on researches with Mülder, more especially in the chemical examination of the tissues while under the microscope. He also about this time came under the influence of the great German physiologist Johann Müller, whose doctrines, more especially those relating to the nervous system and the senses, have largely moulded the life-work, not only of Donders, but of Helmholtz, Carl Ludwig, and many other physiologists in Germany. In these early years, also, he was much occupied with the study of the dynamical characters of living beings, and in a well-known paper on the Metabolism of Tissue as the Source of the proper Heat of Plants and Animals, he showed how the skin acts as a regulator of bodily temperature, and he discussed the relation between heat and work in the living tissues. The fame of Donders largely rests, however, on his researches on vision. In 1846 appeared a paper on the Movements of the Human Eye, and this was followed by many similar contributions through a series of years. In these papers, which were largely devoted to the problems of single vision, the nature of the horopter, the conditions of stereoscopic vision, and the mechanism of the movements of the eyeballs, Donders substantially laid the groundwork of our present knowledge of these subjects. He also wrote on the Relation between Convergence and Accommodation, the Regeneration of the Cornea, and on the Use of Lenses in the Treatment of Squint. After a visit to London in 1851, when he became acquainted with the distinguished ophthalmologist von Graefe, he resolved to devote his life chiefly to this department of the medical art, and for many years he enjoyed a large practice as an ophthalmic surgeon. For twelve years he edited the *Nederlandsch Lancet*, in the pages of which many important communications on ophthalmological subjects appeared from his pen. The great work of his life, however, was a volume entitled *Anomalies of Refraction and Accommodation*, a translation of which was published by the New Sydenham Society. Familiar with the researches of Gauss, Listing, and Helmholtz, Donders investigated mathematically and by experiment the optical conditions of the normal eye, and showed how these were modified in myopia, hypermetropia, and astigmatism. He also discussed theoretically the influence of age upon refraction and the mechanism of accom-

modation, and he gave precision to the optical methods for ascertaining and estimating anomalies of refraction. In all of these researches he not only showed himself to be an able mathematician and physicist, but he enlisted the interest of the medical profession at large by the careful clinical records given of individual cases suffering from anomalies of vision, and by the ingenuity and efficiency of the means devised for their relief. Donders also contributed papers on Physiological Time in Psychical Processes, the Nature of Vowel-Tones, Speech, and the Cardiac Sounds. All his writings are characterised by exactitude of statement, facility in illustration, and graceful diction. The subject is always treated with the hand of a master. Of commanding stature, a dignified presence, a large Apollo-like head with a luxuriant wealth of hair, dark somewhat rugged features, and eyes that sparkled with the lustre of genius, Donders was a man whose personality is not likely soon to fade from the memory. Eminent among physiologists, chief among oculists, a great teacher, and a good citizen, his life-work is thus summed up by his friend Moleschott:—"Of him it would be difficult to pronounce whether he was greater or more prolific as an investigator, or clearer or more effective as an expositor, or, lastly, more duteous and helpful as a healer of that organ which is the portal of wisdom and love."

Rev. James Grant, D.D., D.C.L. Oxon. By **A. Beatson Bell**, Esq., Advocate.

(Read January 5, 1891.)

James Grant was the third son of the Rev. Dr Andrew Grant, proprietor of the estate of Limepotts, in the county of Perth, and minister successively of Portmoak, Kilmarnock, Canongate, Trinity College, and St Andrews, Edinburgh, Dean of the Chapel Royal, and Chaplain in Ordinary to George III., George IV., and William IV.

He was born in the manse of Portmoak, Kinross-shire, on 23rd January 1800, and when James was quite a child, his father was translated to Kilmarnock. He there received the elements of his education, and on his father's subsequent translation to Edinburgh

he was sent to the High School of this city, and afterwards attended the University, both in Arts and Divinity. His career there was marked with distinction, especially in the field of Classics, and in Greek he was a class-fellow and rival of Dr William Veitch of Jedburgh, the learned author of the well-known work on the Greek irregular verb. It was a tradition that, at least in one session, James Grant succeeded in standing first, while Veitch was second in the Greek class.

He was licensed by the Presbytery of Edinburgh, and almost immediately thereafter, in 1824, was ordained minister of the first charge of South Leith. There he remained with much acceptance to the large congregation for nineteen years, notwithstanding some tempting offers of translation. He early was acknowledged as a leader in the Church Courts, and in 1837 was one of a deputation (which included Dr Chalmers and other eminent divines) who presented in person the congratulatory address from the Church of Scotland to Queen Victoria on her accession. In the same year he was one of a deputation sent by the General Assembly to inquire into the religious condition of the people of the Island of Skye.

By this time the troublous events of the "ten years' conflict" had commenced, and Dr Grant was much engaged in its various controversies. Many of these are now matters of somewhat remote history, and it would be improper in a notice such as this to stir embers of former fires. It may be enough to mention that when the party opposed to those with whom Dr Grant acted proceeded to the extreme act of deposition of some ministers in the Presbytery of Strathbogie, Dr Grant and his friends denied the legality of the proceedings, and deliberately visited Strathbogie for the purpose of holding ministerial communion with the deposed ministers. For this ecclesiastical offence the majority of the Assembly inflicted the nominal punishment of suspension from judicial functions for nine months. During his suspension he received a largely-signed address of confidence from his flock, and the same year (1842) the University of Glasgow conferred on him the degree of D.D. At this time also he was appointed chaplain to the Highland and Agricultural Society of Scotland, an honour which he much valued, and which he retained till his death. Next year (1843) saw the end of the ten years' conflict. The parish of St Mary's, Edinburgh,

had become vacant by the demission of Dr Henry Gray, who had gone out with the Free Church, and Dr Grant was offered by the Town Council the presentation. After full deliberation he accepted the translation, and the rest of his active ministerial life was passed in St Mary's. At a later period of the same year he was offered by the Town Council a presentation to one of the charges of the High Church (St Giles), but he declined the offer. He was also then appointed Collector of the Widows' Fund of the Ministers and Professors, an office which he held until 1860, and in which his remarkable talent for business found congenial scope. For ten years after his removal to Edinburgh he was much occupied in a controversy which, in its day, excited bitter feeling, but which is happily now nearly forgotten—viz., the Edinburgh Annuity Tax question, in regard to the manner in which funds for the payment of the stipends of the ministers of Edinburgh were raised. Dr Grant, as one of these ministers, was a prominent figure in the discussions, and ultimately when the controversy was settled by legislation on the footing of the payment of a capital sum by the Corporation of Edinburgh, the interest of which was to take the place of the old tax, and to be applied by a newly-constituted Ecclesiastical Commission, Dr Grant was at once elected a member of that Commission, and ultimately became its chairman. This was the period of his greatest activity, both in parochial work and in the Church Courts, and in 1851 he became Moderator of the General Assembly. The same year he received from Oxford at Commemoration the honorary degree of D.C.L., the only other recipient of that degree among the clergymen of the Church of Scotland having been Dr Chalmers. In 1860 he began to retire from active life outside his parochial work. His attendance in Church Courts almost ceased, and in that year he resigned the Collectorship of the Widows' Fund. In 1871 he resigned his parochial charge, and for the last nineteen years of his life he lived in retirement from active ministerial work, devoting himself much to the management of various religious, charitable, and educational institutions, in the governing bodies of which he held a seat. He was for more than fifty years an Honorary Fellow and Chaplain of the Harveian Society, and at the annual meetings of that body he came in contact with many of the most eminent medical men in Scotland, including many Fellows of this Society.

He became a Fellow of our Society in 1851, and for many years was a regular attender at the meetings, and he served for several years on the Council. Although not himself a scientific worker, he took much interest in hearing of the progress of science in the world, but the papers on the literary side of the Society, then more numerous than of late years they have been, probably had greater attractions for him. He passed away on 28th July 1890, at the ripe age of ninety, preserving his intellect unclouded and his interest in life unabated to the end.

Although he could not be described as a great preacher, his pulpit ministrations were appreciated by his successive flocks, and his kindly interest in their welfare secured the affection of many. Probably his most characteristic quality was his sagacity as a counsellor, whether amid the turmoil of ecclesiastical strife, or, later in life, in the management of the numerous societies and institutions with which he was connected. His memory will be cherished as that of one who realised the dignity of his high profession, and exhibited in his person some of the best qualities of a Scottish clergyman of a school now fast passing away.

Professor Kolbe. By Prof. Crum Brown.

Professor Herman Kolbe was the eldest son of the Rev. Carl Kolbe of Elliehausen, near Göttingen, and was born on the 27th of September 1818. He was educated at home by his father till his fourteenth year, when he entered the Göttingen Gymnasium. In April 1838 he began the study of chemistry, under Wöhler, in the University of Göttingen, where he also acquired a thorough theoretical and practical knowledge of physics and mineralogy under Listing and Hausmann.

In 1842 Kolbe was appointed assistant to Bunsen in the chemical laboratory of the University of Marburg. He took the degree of Ph.D. in that university in the following year, the title of his thesis being "On the Products of the Action of Chlorine on Bisulphide of Carbon."

In the autumn of 1845 he removed to London as assistant to Lyon Playfair. In the spring of 1847 he returned for a short time to Marburg, and in the autumn of the same year removed to

Brunswick, to undertake the editorship of the great *Dictionary of Chemistry,* begun by Liebig and Poggendorff in 1837.

In 1851 he was called to Marburg to succeed Bunsen, who had been translated to Breslau. He remained in Marburg till 1865, when, on the unanimous request of the Faculties of Medicine and Philosophy, he was called to succeed Kühn as professor of chemistry in the University of Leipzig.

In 1870 he added to his professorial work that of the editorship of the *Journal für praktische Chemie.* He died very suddenly, of heart disease, on the evening of the 25th November 1884.

This short notice of the principal landmarks in Kolbe's life has been taken from the full and interesting account given by his son-in-law, Professor E. v. Meyer, in the *Journal für praktische Chemie.*

Perhaps the most striking feature of Kolbe's scientific character was its independence. His opinions and views were his own, and were to an extraordinary degree unaffected by the opinions and theories of others. This independence gives a peculiar value to his theoretical writings, but it had also its disadvantages. The individual character of his style of thinking and writing certainly confined his immediate influence very much to those—comparatively few—chemists who took the trouble to learn his language and understand his methods. The ideas which he originated have now been to a great extent translated into the language of modern chemistry and form part of the common doctrine, but many chemists are unaware of their source, and would scarcely recognise them in the form in which they were first published. The loss to science was temporary, and Kolbe's fame may be safely entrusted to future historians; but the misunderstanding, which was a misunderstanding on both sides, led sometimes to a kind of strife painful to all the friends of those involved in it.

It is not possible, within the limits of an obituary notice, to do more than very briefly indicate the general character of the groups of investigations made by Kolbe, and of the theoretical conclusions he drew from their results.

Kolbe's first great research, published in 1845, was on the compound formed by the joint-action of chlorine and water on bisulphide of carbon. This substance has the composition CCl_4SO_2, and its investigation formed the natural sequence to his graduation

thesis mentioned above. From this "sulphite of perchloride of carbon" Kolbe obtained the potassium salt now represented by the formula CCl_3SO_2OK, and from the corresponding hydrogen salt, by successively replacing the chlorine by hydrogen, $CHCl_2SO_2OH$, CH_2ClSO_2OH, and CH_3SO_2OH.

A great series of investigations, beginning with a joint work by Frankland and Kolbe on the action of caustic potash on the cyanides of the alcohol radicals, led Kolbe to theoretical views as to the constitution and relation to one another of the group of acids now known as "carboxyl compounds" and the corresponding aldehydes, alcohols, ketones, &c. These considerations profoundly influenced the history of chemistry, although, for the reason already mentioned, Kolbe has not even now obtained full credit for what he did. It is true that we are very apt to read old papers in the light of recent discoveries and to find in them more than their writers intended, and the composer of an *éloge* is specially liable to this error; but it is impossible to read Kolbe's papers without seeing that he fully recognised, at a time when no one else had a glimpse of the truth on the matter, what is the real relation of the "sulphone" and "carbone" acids to sulphuric and carbonic acids respectively, and expressed these relations and those of the acids to the aldehydes, alcohols, ketones, &c., with perfect distinctness. His theory enabled him to predict the discovery of the secondary and tertiary alcohols; and when Friedel published his discovery, that acetone gives, by treatment with nascent hydrogen, a propylic alcohol, Kolbe at once declared that this must be one of his secondary alchohols, and that on oxidation it must give, not propionic aldehyde and acid, but acetone, as was soon after found to be the case.

A very early and most interesting investigation by Kolbe, on the "Electrolysis of Potassium Valerianate and Potassium Acetate," belongs to the period of his short residence in London. The most striking result was the synthesis of the hydrocarbons R_2, if we write the potassium salt electrolysed $R{\cdot}COOK$; but the products of the electrolysis were very carefully examined by Kolbe, who detected among them the ethers $R{\cdot}COOR$.

Another important and extensive series of researches bear upon the "oxy-acids." His investigations on lactic acid, and the long and interesting controversy with Wurtz on the constitution of lactic

acid, the synthesis of salicylic acid (by Kolbe and Lautemann), and the reduction of numerous oxy-acids by means of hydriodic acid, carried out by his pupils, belong to this group. In this connection it is right to note that it is to Kolbe that we owe our knowledge of the antiseptic action of salicylic acid.

Besides numerous scientific papers, chiefly published in the *Annalen der Chemie und Pharmacie* and in the *Journal für praktische Chemie*, Kolbe wrote many of the articles in the great *Dictionary of Chemistry*, of which he was editor, and a very valuable *Ausführliches Lehrbuch der organischen Chemie.* In the first and second volumes of this *Lehrbuch* (the only part written entirely by Kolbe) we have a very full account of his views on the constitution of the alcohols, acids, and their derivatives. He also published two short text-books, one on inorganic, and the other on organic chemistry.

James Duncan Matthews. By Professor W. Carmichael M'Intosh, F.R.SS. Lond. and Edin.

(Read January 5, 1891.)

The story of a long life, spent in the service of science, for the most part tells it own tale, and is more or less independent of the biographer; but it is different when a young worker, broken in health, and thus hampered in his efforts, succumbs before reaching middle age.

Born in Aberdeen, Mr Matthews commenced life as an architect in the office of his father (ex-Lord Provost Matthews of Springhill), intending to follow this profession. At the age of nineteen, however, he suffered from a severe attack of typhoid fever, which greatly enfeebled his constitution, and permanently injured his lungs. Though he made several long sea-voyages to Australia and America for the benefit of his health, he only partially succeeded, for the chest-affection continued slowly to progress.

Though in feeble physical health, his active mind was eager for action, and he was led to pursue microscopical work. He then entered Aberdeen University, and studied various biological subjects—especially zoology—which was taught by Professor Ewart, then newly appointed, and with whom a friendship sprung up. Greatly interested in the subject, he resolved to devote his

whole energies to zoological pursuits. His first essay—communicated to the British Association at Southport—was on "Wool Plugs and Sterilised Fluids," a subject for which his conscientious exactness and ingenuity peculiarly fitted him. He concluded by expressing doubts as to the efficiency of wool plugs as filtering agents when a strong current passed through them.

When Professor Ewart was transferred to Edinburgh, Mr Matthews followed him—becoming Demonstrator in Zoology in the University. While in this position, he won the favour of the students and others by his unvarying courtesy, punctuality, and attention to duty. Professor Prince, now of St Mungo's College, Glasgow, who was associated with him in class-work during the summer of 1884, writes of him thus:—"One of the brightest spots in my Edinburgh experience was my daily association with Duncan Matthews, a devoted and unwearied worker amidst all the disadvantages of ill-health and bodily weakness. He was a most accurate and painstaking zoologist, a skilful draughtsman, and was well acquainted with foreign ichthyological literature. Edinburgh never had a more worthy and accomplished, or a more unobtrusive and kindly, professorial assistant. His published papers give no idea of his laborious industry and devotion to zoological work—work which social and other circumstances rendered by no means a necessity."

Along with Professor Ewart he drew up and published a series of directions for the students of the Practical Class on the examination of various invertebrates—similar to those used in University College, London. The critical acuteness of Mr Matthews was well fitted for this work, which, indeed, mainly fell on his shoulders. Yet at this time he seemed to experienced eyes to be on the verge of grave thoracic complications, and one could not but feel for the young assistant gallantly adhering to duty in the absence of his senior when the cold winds of spring told so heavily on his cough. Nevertheless, no complaint fell from his lips, and he performed every task cheerfully and well. He subsequently, however, had to interrupt his labours, and obtain partial relief of the symptoms by a visit to the quiet grounds of Springhill. Next year (1885) he published a very interesting paper on the presence of an oviduct in an adult male skate, besides another series of the joint notes for the

students on the dissection of the skate. These notes gave him even more labour than the previous publication on the invertebrates.

Besides his duties in connection with the class of Zoology in the University, he was employed by the Fishery Board for Scotland to carry on various researches on fisheries' subjects and tabulate results, but he was not responsible for certain of the deductions made from the latter. His singularly clear and cautious mind made him slow to arrive at conclusions—especially in cases fraught with both doubt and difficulty. One of his earlier papers on fisheries' questions was a most careful and methodical report on the sprat-fishing of 1883–84. He accurately pointed out the various distinctions between the sprat and the herring (with the exception of the pelagic egg of the sprat—then unknown), and concluded by demonstrating that in the Firths of Forth and Tay, and in the Moray Firth, the sprat fishermen captured during the winter months 143,000,000 young herrings.

He continued his researches, notwithstanding very feeble health, in 1885, and—his father being then Lord Provost of Aberdeen—he besides took much interest in the arrangements for the meeting of the British Association at Aberdeen in the autumn of the same year. He attended many of the meetings of the Association—especially in Section D (Biology)—though he did not communicate any of his papers. He had much to do, however, in aiding his father in his entertainments at Springhill, and in making his guests (amongst whom were Sir Lyon Playfair, President of the Association, Lady Playfair, and Lord Rayleigh) spend a most pleasant week.

Now fairly entering into the spirit of the fisheries' work, he took up the question of the varieties of the herring. In the skilful hands, and by the exact methods, of Mr Matthews certainty took the place of doubt, and though difficulties still remained, he at any rate reduced the errors from limitation of observation to a minimum. Heincke's paper on the varieties of the Baltic herring did not come into his hands before his own observations were nearly completed, but he was able to make a comparison of the methods. The labour involved in this paper may be estimated when it mentioned that 16,000 measurements and 20,000 subsequent calculations were included, and that the general size, dimensions of the head, differ-

ences in the position of the fins, and other points, were elaborately investigated and tabulated with a tenacity of purpose and innate skill in the manipulation of figures which were prominent characteristics of Mr Matthews. He cautiously concludes this preliminary paper by the statement that the winter and summer herrings slightly differ, viz., in the more posterior position of the fins, the doubtfully smaller head and slightly lesser size of the summer herrings.

About this time he also investigated the kindred subject of the whitebait of the Thames and Forth, and published, in conjunction with Professor Ewart, his results in a short paper. The percentage of sprats and young herrings in these localities is given—the former largely predominating. In the winter fishing of the Firth of Forth the young herrings are practically absent, and in that of the Thames they are in the proportion of only 6 per cent. As the season advances the number of young herrings increases—reaching in May and June 80 per cent. of the shoals, but again decreasing in July.

He continued his persevering researches on the supposed races of the herring in Scottish waters, and issued a second paper on the subject in the Fishery Board's Report for 1887. Here, again, the careful nature of his work, his respect for the observations of others, and his own sound deductions are noteworthy. As the result of his laborious tables and long-continued attention to the subject, he states that there is no true racial distinction between the herrings of the various localities around our coasts, and that the slight differences indicated in his former paper do not—after more extended observations—warrant him in making a distinction. The variations in the position of the dorsal fin during the growth of the herring would alone have rendered the observer careful not to place too much weight on the slight differences formerly indicated, and one can almost sympathise with the earnest young worker who so faithfully plodded through such a mass of materials—skilfully handling every available point—yet with only a negative result as the reward of his free expenditure of labour. He, however, had a talent for figures, and his deductions were always characterised by conscientiousness and exactness.

His investigations on the herring had rendered him familiar with

the anatomy of the fish, and more particularly with the skeleton and its variations. Accordingly, at this time he drew up an elaborate account of the skeleton of the herring, detailing every bone individually and in relation to its neighbours—the whole illustrated by seven plates, for he had a facile and accurate pencil. The amount of patient and monotonous work in this treatise is great, and must with his other labours have taxed the delicate frame of Mr Matthews to a dangerous extent—careful as he was to husband his energies—solely for the advancement of science. The skull, vertebræ, fins, and other parts are minutely described and figured, and the more important differences occurring in the twaite and the allis shad, the pilchard and the sprat, are mentioned. This investigation alone would have entitled Mr Matthews—as a skilful comparative anatomist—to the respect of every zoologist.

This year (1887) was his most prolific one, for in addition to the foregoing laborious papers he produced two others. The first consisted of a report on the examination of 400 stomachs of whiting, the contents of which were carefully tabulated. His observations led him to believe that the whiting fed for the most part on small fishes and crustaceans, thus differing to some extent from the cod and haddock, both of which had a more varied dietary. The second paper gave an account of the nest, eggs, and newly-hatched larvæ of the Ballan Wrasse (*Labrus maculatus*) from Broadford in Skye. Mr Matthews was thus the first observer who recorded this feature in our country.

The efforts of 1887 just recorded, and of the previous years—when he several times lectured for Professor Ewart, as well as conducted the class of Practical Zoology—proved too severe a strain, and he had to retire to Springhill, his quiet walks amidst the beautiful gardens and grounds of which had formerly restored a measure of health. There, as his brother-in-law, Dr Ogston, tells us, he lived amongst his specimens and aquaria—"converting his rooms into extemporised workshops and laboratories, where his investigations were carried on. As his strength waned, these grew more intermittent, but even to the latest hour of consciousness he remained surrounded by his plants and animals, and showed his interest in them." A journey to London to attend the funeral of his uncle, Dr Matthews Duncan,

a kindred spirit, and to whom he was tenderly attached, dangerously exhausted him, and since that time his strength gradually diminished. He died on the 24th November 1870 at the age of thirty-nine years.

Mr Matthews had a singularly clear, well-balanced, and vigorous intellect, keen observation, and remarkable powers of application. There is, indeed, no doubt that he would have achieved an eminent position in science if his health had been favourable. As it was, he became one of the best authorities on the clupeoids, and no one took more interest in the group. Even when confined to bed, and unable to do more than write briefly in pencil, he perseveringly tried to secure anchovies, then appearing here and there on our coasts, so that fresh observations on this form might be carried out.

Taken as a whole, the career of Mr Matthews is an instance of exemplary devotion to duty—under great physical difficulties—in a field he had deliberately chosen. Many men in his position would have felt the weight of physical illness sufficient to bear, and would have passed their valetudinarian hours in search of ease and repose. Not so with Mr Matthews. Like Edouard Claperède of Geneva, he even adhered to his labours after repeated hæmoptyses—preferring "rather to wear the sword out than let it rust out." The hand of the gentle young naturalist has vanished, but his accurate work will remain as a proof of his resolute perseverence under difficulties, and of his loyalty to zoological science.

Memoir of Colonel Sir Henry Yule, R.E., C.B., K.C.S.I., LL.D., &c. By Coutts Trotter.

(Read January 5, 1891.)

When the Royal Society of Edinburgh, in 1883, conferred the distinction of an Honorary Fellowship on Colonel Henry Yule, they were moved thereto, probably, as much by the wide range of subjects felicitously touched by his genius, as by the rare quality of the work done by him in his special domain of Comparative Geography.

The difficulty of adequately handling these numerous and

varied topics, and still more of doing justice to the personal qualities of the man, within the space usually allotted to such a notice, increases the hesitation I have felt in complying with the wish of the Society that I should undertake the task; and, in so far as I owe this honour to my intimate friendship with Henry Yule, I am not reassured when I recall the many eminent and representative men with whom I shared that privilege.

The youngest son of Major William Yule of the Indian Army (himself a devoted student of Oriental literature), Henry Yule was born at Inveresk on the 1st May 1820. The family had for many generations previously held a leading place among the well-to-do farmers of East Lothian, being settled in the parish of Dirleton, where they also owned some land; and many of them lie buried in the old church of Gullane. The subject of this memoir had two brothers, who both distinguished themselves in India—Sir George, a very able and popular civilian, and Robert, who fell fighting at the head of his regiment, the 9th Lancers, before Delhi, during the Mutiny. The family is said to be of Danish origin, the name, spelled Jul, or Juul, being still not uncommon at Copenhagen.

Henry Yule was at first intended for Cambridge, and probably for the Law, for after leaving Edinburgh he was placed successively under two mathematical tutors—Hamilton, author of *Conic Sections* and subsequently Dean of Salisbury, and Challis, afterwards Plumerian Professor at Cambridge. His fellow-pupils here were the late Rev. Dr John Mason Neale, and Dr Harvey Goodwin, the present Bishop of Carlisle. The latter, to whose kindness I am indebted for these reminiscences, says that Yule "showed much more liking for Greek plays and for German than for mathematics, though he had considerable geometrical ingenuity." That he had this seems, indeed, pretty clear from the fact that on one occasion he solved a problem which had puzzled the future accomplished mathematician who tells the story. Yule's comment on the matter, addressed to Goodwin, being—"The difference between you and me is this, you like it and can't do it; I don't like it and can do it." He added "Neale neither likes it, nor can do it."

His having to leave Mr Challis, who could no longer accommodate him as a pupil on removing to Cambridge, may have led

Yule to reconsider his future course and abandon Cambridge for an Indian career.

In 1837 he went to the Indian Military College of Addiscombe, and passing out thence at the head of his term was appointed, in 1840, after a year's residence in Chatham, to the Bengal Engineers. Whilst at Chatham, as his contemporary General Collinson writes :* "Although he took small part in the games and other recreations of our time, his knowledge, his native humour, and his good comradeship, and especially his strong sense of right and wrong, made him both admired and respected."

His earliest Indian appointment, among the Khasias, a primitive Mongoloid people on the north-east outskirts of Bengal, is interesting as having led to the first of his many quaint and curious notices of remote Eastern peoples.†

Another literary memorial of his early days, evidence already of the literary instinct, was a volume on "Fortification," ‡ written while at home on furlough (1849–51), and lecturing on the subject at a long-vanished Edinburgh institution—the Military Academy. "It may still," his brother engineer writes, "be read with benefit;" while for the general reader its interesting biographical notices and portraits of famous engineers make it very unlike the ordinary professional treatise. A French translation appeared in Paris in 1858.

Henry Yule had previously, in 1843, been at home on leave, when he was married to Anna Maria, daughter of General Martin White of the Bengal Army; and on his return to India in 1852, after his second furlough, his wife, owing to bad health—the result of an accident soon after their marriage—was unable again to accompany him. Between 1843 and 1849 he was serving with that group of distinguished engineer officers—among whom we recall the names of Baird Smith, Cautley, W. E. Baker, Napier (afterwards Lord Napier of Magdala), and Richard Strachey (the last named the only survivor)—then engaged on that great and successful enterprise, the restoration and development of the irrigation system of the Mogul dynasty in the North-West Provinces.

* *Royal Engineers' Journal*, 1st February 1890.

† *Journal of the Asiatic Society of Bengal*, vols. xi. and xiii.

‡ Published by Wm. Blackwood & Sons, 1851.

These labours were interrupted in 1846 and 1848 by the first and second Punjab wars, in both of which he saw active service.

The confidence already placed in him by Lord Dalhousie was shown by his appointment to an important post in connection with the great scheme of Indian railways which that statesman had just introduced, and was pressing on with characteristic energy, a post entailing, from its novelty, much hard and anxious study; and this led on to his appointment as Under-Secretary in the newly-established department of Public Works, to the head of which he succeeded on the retirement, after the Mutiny, of his friend and chief Sir W. Baker. In the meanwhile, during the Burmese war, he was despatched to survey the frontiers of Arakan, when he acquired the friendship of Sir Arthur Phayre, a capable and justly popular administrator, whose name will always be honourably associated with Burmese affairs. This acquaintance, no doubt, led to the appointment of Captain Yule as secretary to the return mission of reconciliation despatched to Burmah in 1855, after the war, under Sir Arthur Phayre. It was stipulated, however, by Lord Dalhousie that Yule should be the chronicler of the expedition, a promise amply fulfilled in a Report to Government, which was afterwards re-cast and published by Smith & Elder in 1858, and is interesting to us as his first independent work of importance.

It has been said that his attention was first directed by his Burmese journey and studies to the affairs of those regions beyond India, on which he afterwards became so great an authority; but the truth is, we find much of the knowledge, and of the literary intuition, here already, and even thus early in his career we wonder at the variety of information displayed, and the luminous generalisations put forth, both alike destined—and this is no common praise—to stand the test of later and fuller investigations, now so much more easy to make. The illustrations are mostly from his own pencil, in the use of which he was no mean proficient.

The work, compiled amid the absorbing labours of his Calcutta office, was finished, to judge by the concluding sentence of its preface, amid still more engrossing scenes. It is dated "Fortress of Allahabad, October 3, 1857."

"If life be granted, I doubt not all my companions in the Ava Mission will look back to our social progress up the Irawadi, with

its many quaint and pleasant memories, as to a bright and joyous holiday; which, indeed, it was. But for one standing here on the margin of these rivers, which a few weeks ago were red with the blood of our murdered brothers and sisters, and straining the ear to catch the echo of our avenging artillery, it is difficult to turn the mind to what seem dreams of past days of peace and security; and memory itself grows dim in the attempt to repass the gulf which the last few months has interposed between the present and the time to which this Narrative refers."

He visited soon after these tragedies the historical Well of Cawnpore, and afterwards designed the erection which encloses it.

On his way home from Burmah he was sent to report on the defences of Singapore, and the works he recommended were sanctioned by Government.

Although residence in India during the latter years of his stay became in some degree distasteful from various causes—among others the prolonged absence from wife and child, and considerations of health—he enjoyed the compensation of feeling that his character and services were appreciated in the highest quarters, for the confidence and regard, so fully and heartily bestowed by Lord Dalhousie, were continued in no stinted measure by Lord Canning; the intimacy becoming naturally greater, for such a crisis as the Mutiny brings out the deeper qualities of good men, and reveals them to each other. His admiration for Lady Canning, and regret for her loss—a victim to the anxieties of that terrible time—are recorded in some touching lines to the memory of that charming and gifted woman. He retired from the service in 1862, with the less hesitation that Lord Canning, who was then returning to England, had given him the confident assurance that he should receive some suitable employment. And no doubt this would have been the case had Lord Canning lived. But, as may be remembered, he died almost immediately after his return, and even had he left any political heir, Colonel Yule would have been the last man to urge his own claims. Full, however, of sympathy and interest, personal as well as public, in his late chief's career, he was desirous to write his life. But the family declined his offer, which is to be regretted, not merely because the various short memoirs he has since compiled are models of what such essays ought to be, but also

because the deliberate utterances of such a man, treating, as he must have done, some of the more important Indian topics of the day, would have had much interest and value.*

The harsh stroke of fortune, by which he was denied professional employment, was a gain to literature, for the period of thirteen years which elapsed before he found himself again in official harness produced, with other important work, the book on which his literary reputation chiefly depends, viz., the translation and editing of *Marco Polo.* It seems safe to prophecy a lasting reputation for this work, since it is hardly conceivable that editing could be better done, and its appearance in 1870 placed its author by common consent, here and abroad, in the very front rank of the geographers of his time. Yet its appearance was not, strictly speaking, a surprise. The nature and extent of the writer's learning was already known by various essays on allied subjects, and notably by a work written for the Hakluyt Society four years previously, entitled *Cathay and the Way Thither.* This important work, long out of print and practically inaccessible, contains a fund of curious information on mediæval Asia, and on the relations from earliest times between China and the West, more especially during the period of Chinese exclusiveness which intervened between the fall of the Mongols and the arrival on the scene, two centuries later, of the Portuguese and Spaniards. But the Hakluyt Society addresses a limited class of readers only, while *Marco Polo,* alike from the romance which still clings to the old traveller's name, and from the quaint illustrations and other excel-

* His principal biographical notices are of Major James Rennell, R.E., the geographer; General A. C. Robertson; General Sir W. E. Baker (written jointly with General R. Maclagan); General W. A. Crommelin; General W. W. Greathed; and Colonel George Thomson. The last-named officer is known to fame as having performed the feat which led to the fall of Ghuznee. Accompanied by two subalterns, under a heavy fire, he carried a bag of gunpowder to the gate of the fortress and blew it in, enabling our troops to enter. A leading newspaper, writing his obituary, stated that Thomson "was present at the capture of Ghuznee," on which Yule characteristically comments—"The fact will hardly be controverted; we believe it is also true that Todleben was present at the defence of Sebastopol." These are all in the *Royal Engineers' Journal.* A notice of Sir Arthur Phayre is in the *R.G.S. Proceedings* for 1886. There is also a very curious notice of George Strachan, an early Persian traveller, in the *Asiatic Quarterly Review,* v. 10.

lences of the volume, appealed to a far wider circle. Not the least among the merits and attractions of this famous book is the style of the translation itself. It is archaic, and yet living, and instinct with the very spirit of the old Venetian. The translator has lived so long with him and his contemporaries that, while always his editor, and wielding the accumulated knowledge and diacritic faculty of these later days, he is in intimate sympathy with these brethren five hundred years his juniors. How thorough-going the intimacy, is sufficiently shown by the preface, written in fourteenth-century French, to the second edition, and which some matter-of-fact readers, though greatly puzzled, are said not to have discovered to be a *jeu d'esprit.*

A fashionable London lady, otherwise imperfectly posted, once addressed Sir Henry Taylor as "Mr Van Arteveldt;" and Colonel Yule's popular identification with his hero was hardly less complete, and he enjoyed it, and often signed occasional letters to the papers with the initials "M. P. V." (or "Marcus Paulus Venetus").

But beyond even this spirit of discriminating sympathy, as giving value to these works, were his remarkable thoroughness and accuracy, the outcome of a scrupulous and uncompromising honesty, and an "infinite capacity for taking pains." And with sympathy, accuracy, and memory—and Colonel Yule had a marvellous memory—the diligent scholar is already far on his way.

It is characteristic of him that he takes a personal satisfaction in rehabilitating the reputation for accuracy, and anyhow the truthfulness of intention, not only of the great "Marco Milione," but of such lesser lights as Friar Odoric, Marignolli, and others, among the mediævals; while with equal shrewdness and generosity he defends the Abbé Huc against the strictures of Prejevalski,* reasoning acutely enough that his amusing and phenomenal lack of science was no proof of bad faith or dishonesty. For all these travellers, as workers in his own *Fach*, have his special sympathy, and he, with a wider grasp than others of their special difficulties, is the more ready to make allowance for them. He had a strong feeling, not only that all such work should be done as well as possible, but

* *Mongolia*, &c., by Lieutenant-Colonel N. Prejevalski. Translated from the Russian by E. D. Morgan, with introduction and notes by Colonel Yule.

that it was in itself important, as adding to the wealth of the world. It is difficult to think of him as ever asking "*Cui bono?*"

He has been accused of being deficient in a sense of literary proportion. The chief ground for the charge, and perhaps its excuse, may be found in his extraordinary fulness of knowledge, always at hand and ready to come forth. He used often to laugh at (what cynics might call) its "uselessness," and could quite enjoy the charge from its humorous side. But his whole heart was in his subject as he wrote, and his conviction of the importance of his subject infects the reader, whose judgment at the same time is gained by the assurance which comes to him of the truthfulness and appositeness of the references, the comparative values of which, besides, have meanwhile been all worked out for him.

It would be beyond the scope of this paper to institute a comparison between our present knowledge of the physical geography, and of the condition in mediæval times, of Central Asia, and what was available, under either head, in Colonel Yule's younger days. It sounds like exaggeration to compare Central Asia before Yule, with Central Africa before Livingstone; but the comparison is less far fetched than might be supposed. And even now, notwithstanding the extensive labours of recent explorers—often carrying Yule's *Marco Polo* in their hands, and always revolving in their minds some problem he has suggested or illustrated—there remain vast tracts virtually unknown, and some great hydrographic questions only partially solved. And, as in respect of the geography, so too in the mediæval history and archæology; the awakening of interest, and the direction of research, are largely due to the influence of these works. The sources of knowledge existed, indeed, before, but they were remote and unfamiliar, and above all undigested; it needed his intuitive power of sifting and collating, separating the wheat from the chaff, while throwing a glamour of interest over all, to make such a subject at once intelligible and popular. I may be allowed to confirm this estimate by some words of Baron F. von Richthofen, who holds the very first rank in his own country as at once an enterprising and scientific traveller, and a man gifted with wide philosophic observation. Not only in England, he says,

"aber auch in den Literaturen von Frankreich, Italien, Deutschland und anderen Ländern ist der mächtig treibende Einfluss der Yule'schen Methode, welche wissenschaftliche Gründlichkeit mit anmutender Form verbindet, bemerkbar." And after some touching words on the personal character of the man, he emphatically accords him "den unbestrittenen Platz als des ersten Vertreters und Bahnbrechers unserer Zeit auf dem Gebiet der historischen Geographie." *

I may cite, further, a letter from Mr Delmar Morgan, who, not only as a scholarly writer and an expert in Asiatic geography, but also from his position on the Hakluyt and Asiatic Societies, is specially conversant with Colonel Yule's work. He dwells on "his rare skill in making intelligible to his readers the most perplexing and confused accounts of geographical explorations, the thorough way in which he mastered his authorities, and knew from a conscientious study of their works how much reliance was to be placed on them. Take his *Marco Polo* and open it at any page, and you will find as much learning in a single note as some writers are content to put into a chapter, or even a volume. . . . . Lastly, in all he wrote and all he did he was always A1. Nothing second-rate or mean emanated from him." The veteran geographer, Mr H. W. Bates, F.R.S., also writes to me expressing himself emphatically in the same sense, both from the literary and from the moral point of view; and those who know him will not question the competency of his judgment in either particular.

The labour of compiling such a work as *Marco Polo* was enhanced by a compulsory residence at Palermo, where he had taken up his abode in 1864 on account of his wife's health. The comparative nearness to the great Italian libraries was, however, an assistance; and in truth his references were drawn from every corner of the world, and he had occasionally to wait for months for the verification of a single statement by a correspondent, in the heart, maybe, of China or Tartary. But such labour was repaid by a correspondence often of great interest, and by the acquaintance, not seldom ripening into fast friendship, of many distinguished men of various countries, and not the least of Italy, where such an

* *Verhandlungen der Gesellschaft für Erdkunde zu Berlin*, Bd. xvii. No. 2.

edition of her famous traveller was received, as was natural, with warm appreciation.*

Of the remarkable intuition with which he was wont to resolve a geographical puzzle, a single instance—as it deals with a curious piece of geographical scandal—may be quoted. In the years following 1860 the countries lying between the then Russian frontier and our own were the object of very keen interest to geographers, the political rivalry underlying this interest being not less keen. Accordingly, much surprise was felt in this country at the discovery that there existed at the Russian War Office a narrative of exploration in those countries by a certain German baron, said to have been in the employ of the Indian Government. The authenticity of the narrative was warmly maintained by Russian geographers, but it was proved that no such person as the traveller in question had been in the service of the Indian Government, and on other counts the narrative was pronounced here, by Sir H. Rawlinson and Lord Strangford, to be a forgery.

It was, however, a circumstantial story, and its geography agreed with the map published from Jesuit sources by Klaproth. Along with this document there was another, purporting to be a translation (by Klaproth) of a Chinese traveller; while a collection of papers of similar tenure, which had been sold by him for a large sum to our Foreign Office, came to light about the same time. Colonel Yule, on close examination of the positions of places in Klaproth's map, observed a uniformity of error founded evidently on some principle, and finally discovered that certain of the separate squares on which, according to the Chinese practice, the map had been originally drawn, had first been omitted, putting thereby the longitude of places to the west of the *lacuna*, so caused, too far to the east; then, the error having been discovered, the missing portion had been inserted, and again a certain uniformity of error appeared in the positions; and this last time he discovered that this portion of the map, when being inserted, had been accidentally turned round in an angle of 90° (making east north, and north

* The *Bollettino della Società Geografica Italiana* for March 1890 contains an eloquent tribute of affectionate regard, along with a very high estimate of Henry Yule's geographical achievements, from the very competent pen of Professor Giglioli.

west, and so on), an accident possibly due to the fact that on Chinese maps the names are written perpendicularly instead of horizontally. And, in conclusion, as the positions thus falsified in the map agreed with those given to the places in the narratives in question, the latter were evidently fictitious, and but too clearly from the pen of the able geographer who was probably the only person capable of having concocted them!

The errors, honest enough as far as the map was concerned, affected our own atlases during many years.*

The introduction by Colonel Yule to Captain Gill's *River of Golden Sand* † brings a mass of lucid research to bear on the vast river system which, originating on the plateaux of Eastern Tibet, sends its streams either eastward through China, or south, in long parallel courses, to the Indian Ocean. The whole question, of great difficulty owing to the inaccessible and little known character of the region, had occupied his mind for many years; the distinguished French explorer of the Mekong, Francis Garnier, being one of his many sympathetic correspondents.

After his wife's death, in 1875, Colonel Yule returned to England, where he was very warmly welcomed, and was at once placed on the Indian Council. Although not many years after this he was attacked by the wasting disease to which he eventually succumbed, we find but little diminution, up to the last, in his recorded work, while the amount of unrecorded work, friendly help given, often under the heavy pressure of physical prostration, to the literary labours of others, was very great; his keen appreciation, in fact, of such labour attracted his sympathy irresistibly to the workers themselves. And his interest in all else that life had to offer—in art, in politics, in discovery, in social and philanthropic movements, in the welfare of his friends—continued to the last unabated.

His second marriage, in 1877—to Mary Wilhelmina, daughter of Mr Fulwar Skipwith, late of the Bengal C.S.—brought into his life an episode of hardly four years' domestic happiness, unclouded, save by the anxiety caused by his wife's delicate health; and he lost

* See *Journal of the Royal Geographical Society* for 1872, vol. xliv., and Introduction to Wood's *Journey to the Source of the River Oxus*, new edition, 1872. Murray.

† Murray.

her just when his own health was declining, and his need of such companionship the greater.

Among other subjects of interest to him in these latter years, the Hakluyt Society had naturally a prominent place, its objects corresponding closely to the line which he had made more especially his own; and not a few of the merits of various works by others in that series have been due, as their authors would willingly admit, to the help he so ungrudgingly gave. His own last work in the series, *The Diary of Sir William Hedges*, to which he devoted much of his latest energies—poured out like the profuse flowering of a dying tree—overflowed into a third volume, which contains, *inter alia*, a mass of curious documentary material towards a biography of Thomas Pitt, grandfather of the first Lord Chatham, and of "Pitt Diamond" celebrity—the story of that famous stone being given at length. It was but very shortly before his death that he resigned the Presidency of this Society, and sent for his accomplished fellow-labourer, Mr Clements Markham, to express the hope that he would succeed him there. He was, naturally, an honoured member and Gold Medallist of the Royal Geographical Society, and an Honorary Fellow of our own Scottish Geographical Society. He received the LL.D. degree from the University of Edinburgh at its tercentenary commemoration. He was also President, till his health failed, of the Asiatic Society, and was wont to urge its claims for support from all interested in our Eastern Empire.

He was always on the look out for fresh materials for a second edition or supplement to his *Glossary of Anglo-Indian Colloquial Words and Phrases, and of Kindred Terms*,† which appeared in 1886, the compilation of which, apart from the sense of exhaustion which work produced, had been to him, as he says in the touching dedication to his brother, "trium fermè lustrorum oblectamentum et solatium." Each of the terms is used as a peg whereon to hang the quaint medley of illustrations and references collected in his miscellaneous reading, and stored till wanted in the chambers of an unfailing memory. The book was begun in connection with Dr Arthur Burnell, and owes much to his great philological knowledge; but he died soon after it was commenced, and some seven-eighths of the volume is Colonel Yule's. The book is far less known, and its merits less appreciated, than they deserve to be.

In the *Encyclopædia Britannica* his articles on "Sir John Mandeville" and on "Prester John" are exhaustive, and good examples of his style of work, and the paper on "Lhasa" is also valuable.*

He was singularly happy in the composition of monumental and other inscriptions; and, in a very different line, in his poetical effusions, sometimes grave, but others highly humorous, and occasionally in good Scotch. He had begun shortly before his death to collect his fugitive pieces, with sketches and photographs illustrating them, and it is to be hoped some instalment of these may see the light before long.

The humorous verses, and the inscriptions referred to, recall two marked features in his character; a deep seriousness, with occasional despondency, and for antidote a keen and delightful humour, never far from the surface in his conversation or his writings. And he appreciated humour in others, so long as it was free from cynicism or unkindliness; this revolted him, for he had great delicacy of feeling, and a warm and tender heart, with a ready sympathy for real sorrow, though small patience for the unreal or conventional. If he was vehement in assertion, and scathing in denunciation of all that to him seemed mean, or false, or unjust, this came mainly from the old Scottish sense of the seriousness of life, and of the importance, in all things, of being on the side of truth and right. For personally his simplicity and humility were alike marked and touching, though his presence had all the personal dignity of one who knew he had long and steadily followed a lofty ideal. He had a large capacity for friendship, and in his rooms the walls, and even the doors, were covered with the portraits of his principal friends, as also with a very interesting and complete collection he had made of portraits of the Governors-General and Commanders-in-Chief of India.

It was only a few months before his death that he resigned his place on the Indian Council, which the kindness and consideration of his colleagues had enabled him to retain far longer than he could otherwise have done. Here his services had long been valued, not only for the extent of his knowledge, and his clearness of perception, but from the spirit and tone in which he was wont to

* For a fuller list of his contributions to literature, see the *Scottish Geographical Magazine* for February 1890.

handle public questions. And, indeed, by many who knew but little of him from the scientific side, he will be long remembered as an example of chivalrous integrity, and for his consistent and often fiery protests against all that was unworthy and base.

As a striking instance of the clearness of a strong mind amid the final prostration of the body, I may quote the dying reply—instinct with more than the old Roman's dignity, while resting on a higher faith—which he dictated in Latin to the French Académie des Inscriptions et Belles Lettres, which had just made him a Corresponding Member.

"Reddo gratias, illustrissimi domini, ob honores tanto nimios quanto immeritos. Mihi robora deficiunt, vita collabitur, accipiatis voluntatem pro facto. Cum corde pleno et gratissimo moriturus vos, illustrissimi domini, saluto.—YULE."

The following sympathetic commentary on these words appeared in the *Academy* of March 29, 1890, over the signature "D. M.":—

"*Moriturus vos saluto.*"
Breathes his last the dying scholar—
Tireless student, brilliant writer;
He "salutes his age," and journeys
To the undiscovered country.

There await him with warm welcome
All the heroes of old story—
The Venetians, the Ca Polo,
Marco, Nicolo, Mappeo,
Odoric of Pordenone,
Ibn Batuta, Marignolli,
Benedict de Goës—"Seeking
Lost Cathay and finding heaven."
Many more whose lives he cherished,
With the piety of learning;
Fading records, buried pages,
Failing lights and fires forgotten,
By his energy recovered,
By his eloquence rekindled.

"*Moriturus vos saluto.*"
Breathes his last the dying scholar,
And the far-off ages answer:
"*Immortales te salutant.*"

He died at his residence in London on the 30th December 1890.

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